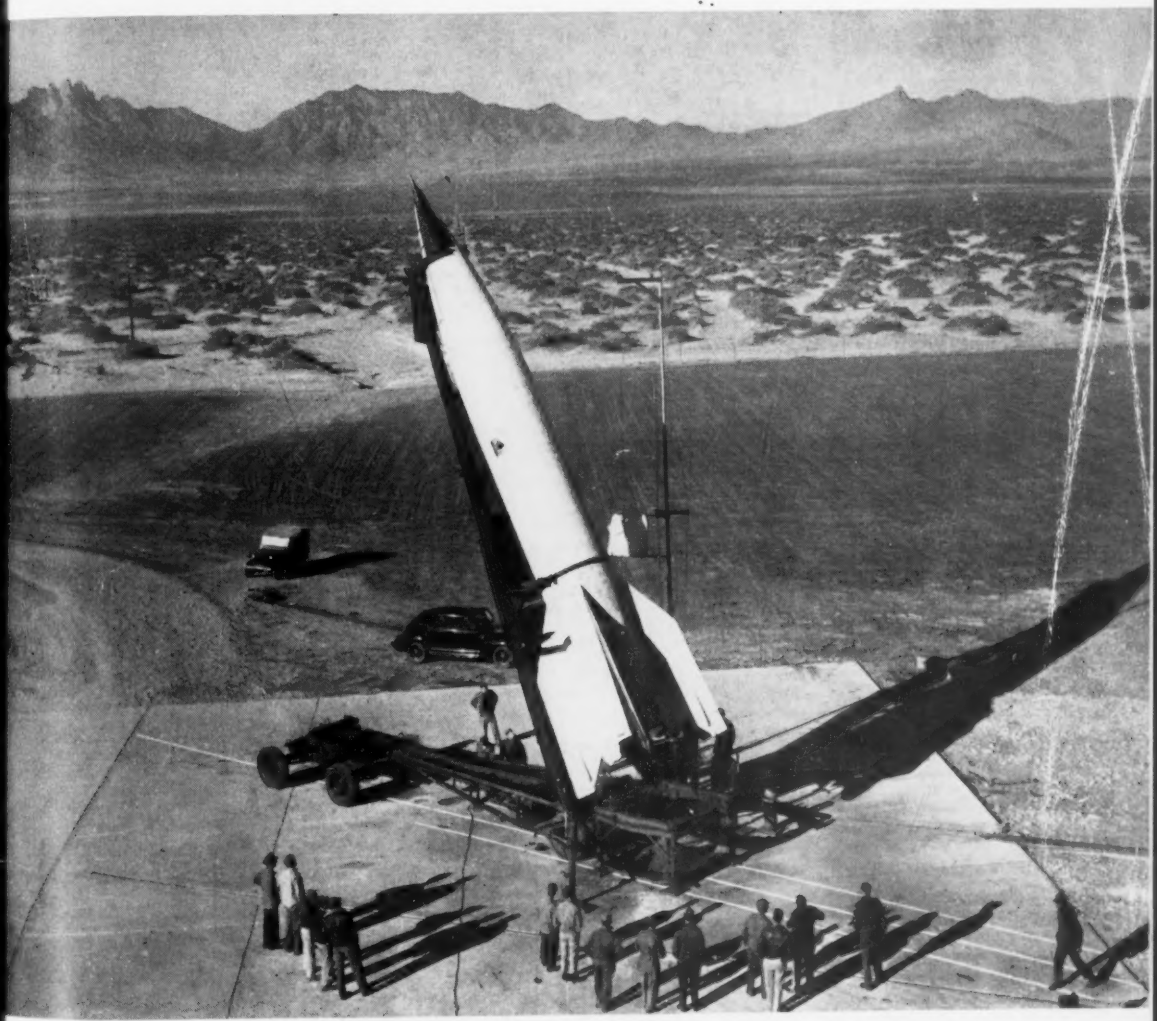
JUN 30 1947

The SCIENTIFIC MONTHLY

PERIODICAL ROOM
GENERAL LIBRARY
UNIV. OF MICH.

VOL. LXIV *June 1947* NO. 6



RAISING A ROCKET TO THE LAUNCHING PLATFORM

See *Exploration of the Upper Atmosphere by Means of Rockets*, page 453

PUBLISHED BY
THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

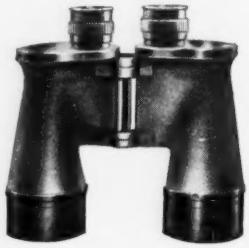
BARGAINS in WAR SURPLUS LENSES & PRISMS

ASSEMBLE YOUR OWN BINOCULARS!

Complete Optics! Complete Metal Parts!

Save More Than ½ Regular Cost

GOVERNMENT'S 7 X 50 BINOCULARS



Here's an unusual opportunity to secure a fine set of Binoculars at a substantial saving of money. Offered here are complete sets of Optics and Metal Parts for the Government's 7 X 50 Binoculars (water-proof model). These components are new and all ready for assembly. We supply full instructions. Limit—1 set of Metal Parts and 1 set of Optics to a customer.

METAL PARTS—Set includes all Metal Parts—completely finished—for assembly of 7 X 50 Binoculars. No machining required. Bodies have been factory hinged and covered. Waterproof Gaskets are included. A sturdy Binocular Carrying Case is included with each set of Metal Parts.
Stock #840-X ..... 7 X 50 Metal Parts ..... **$35.00 Postpaid**

OPTICS—Set includes all Lenses and Prisms you need for assembling 7 X 50 Binoculars. These Optics are in excellent condition—perfect or near perfect—and have new low reflection coating.
Stock #5102-X ..... 7 X 50 Optics ..... **$25.00 Postpaid**

NOTICE! Add 20% Federal Excise Tax to above prices if you order *both* Binocular Optics and Metal Parts.

ARMY'S 6 X 30 BINOCULARS

No Carrying Case with any Sets shown below. (None yet available in Surplus Market.) M-13A1 sets are waterproof model. M-3 sets are not waterproof. Limit—1 Set to a Customer on all Sets shown below.

COMPLETE OPTICS & METAL PARTS—Model M-13A1, 6 X30 Binoculars. Everything you need—ready for assembly. When finished will look like a regular factory job costing $102 to $120. The Optics are new, in perfect or near-perfect condition. Have new low reflection coating. Metal Parts are new and perfect, all completely finished. No machining required. Bodies factory hinged and covered. Complete assembly instructions included.
Stock #830-X ..... **$40.00 Postpaid, plus $8.00 tax—Total—$48.00**

COMPLETE OPTICS & METAL PARTS—Model M-3, 6 X 30 Binoculars. The Optics in this set are new, perfect or near-perfect. Prisms have new low reflection coating. Factory mounted Eye Piece and Objective Assemblies not coated. Metal Parts are perfect, new, ready for assembly. When finished, this will look like a regular factory job, except a name has been filed off a cover plate. No machining required. Bodies factory hinged and covered.
Stock #831-X ..... **$35.00 Postpaid, plus $7.00 tax—Total—$42.00**

METAL PARTS ONLY—Model M-13A1, 6 X 30 Binoculars. No Optics. Same Metal Parts as described for Stock #830-X.
Stock #832-X ..... 6 X 30 Metal Parts ..... **$25.00 Postpaid**

METAL PARTS ONLY—Model M-13A1, 6 X 30 Binoculars. No Optics. Same Metal Parts as described for Stock #830-X. Bodies hinged and Prisms Shelf holes placed, but you must tap them. Prism Shelves have been machined. Six lead spiral focusing threads have been cut. Some less difficult components you must thread and machine yourself, but all material you need is furnished except body covering material and Optics.
Stock #833-X ..... 6 X 30 Metal Parts ..... **$12.00 Postpaid**

OPTICS FOR 6X30 BINOCULARS (No Metal Parts). Slight seconds. Cemented but not coated.
Stock #5123-X ..... **$10.00 Postpaid**

SAME OPTICS AS ABOVE (6 X 30), but coated.
Stock #5124-X ..... **$12.75 Postpaid**

NOTICE! Add 20% Federal Excise Tax to above prices if you order *both* Binocular Optics and Metal Parts.

Optics and Metal Parts are Available for Monoculars (½ a Binocular). For Details Write for Bulletin #14-X.

Order by Set or Stock No.

OPTICAL BENCH KIT—(Not a War Surplus Item). Everything you need, including four Lens Holders for checking Focal Lengths and setting up Lens Systems.
Stock #53-X ..... **$8.50 Postpaid**

23 POWER PRISM SPOTTING TELESCOPE SET—Optics consist of perfect Achromatic Objective with dia. of 64 mm. and 20 inch F.L. (not a War Surplus item), an Eye Achromat, Field Lens and Prisms from 6 x 30 Binocular. These are slight seconds. All low reflection coated except Objective. Half a covered 6 x 30 Binocular Body, all parts for Eye-Piece end and Prism Shelf section are supplied. You must furnish an extension tube and any parts needed to mount the Objective Lens.
Stock #5127-X ..... **$21.00 Postpaid**

BOMBER SIGHTING STATION—A double end Periscope Type Instrument of highest precision. Brand new and in perfect condition. 6 ft. tall, shipping wt. 360 lbs. Orig. cost $9,850. Consists of numerous Lenses, Prisms, Mirrors, Gears, Motors, Metal Parts and Electrical Gadgets.
Stock #914-X ..... **$50.00 F.O.B. Oklahoma**

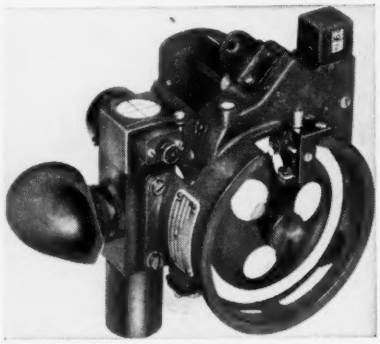
AIR FORCES GUN SIGHT—With Polarizing Variable Density Attachment. (Polarizing attachment alone is worth many times the price of entire unit.)
Stock #908-X ..... **$5.00 Postpaid**
Same Unit Without Polarizing Attachment
Stock #916-X ..... **$2.50 Postpaid**

SCHMIDT OPTICAL SYSTEM—Black plastic body, size 3¼" by 5¼". F.L. 2.4". . . . . with amazing speed of F.O.9. Used in Navy's Infra-Red Sniperscope and Signalling Units. Govt. cost $134. Limit—1 to a customer.
Stock #720-X ..... **$6.00 Postpaid**

BATTERY COMMANDER'S PERISCOPE With Tripod—6 Power Instrument. Excellent condition. Length 27½ inches-diam. 1½ inches. Cost U.S. Govt. approximately $175.00.
Stock #717-X ..... **$20.00 F.O.B. Audubon**

SPECTROSCOPE SETS . . . These sets contain all Lenses and Prisms you need to make a Spectroscope Plus FREE 14-page Instruction Booklet.
Stock #1500-X—Hand Type ..... **$3.45 Postpaid**
Stock #1501-X—Laboratory Type ..... **$6.50 Postpaid**

NEVER HAS THERE BEEN SUCH A SENSATIONAL BARGAIN AS THIS!



BUBBLE SEXTANT—Type A-10, used by Army Air Forces. These have just arrived and will be priced so low you'll think we have made a mistake. Send at once for complete descriptive Bulletin 16-X containing pictures, facts and price. If interested, don't wait, because this bargain won't last long.

RAW OPTICAL GLASS—An exceptional opportunity to secure a large variety of optical pieces, both Crown and Flint glass (seconds) in varying stages of processing. Many prism blanks.
Stock #703-X 8-lbs. (Minimum weight) ..... **$5.00 Postpaid**
Stock #702-X 1½ lbs. ..... **$1.00 Postpaid**

TANK PRISMS—PLAIN OR SILVERED
90-45-45 deg. 5¾" long, 2⅛" wide, finely ground and polished.
Stock #3004-X—Silvered Prism (Perfect) ..... **$2.00 Postpaid**
Stock #3005-X—Plain Prism (Perfect) ..... **$2.00 Postpaid**
(Illustrated Book on Prisms included FREE)

WE HAVE LITERALLY MILLIONS OF WAR SURPLUS LENSES AND PRISMS FOR SALE AT BARGAIN PRICES. WRITE FOR CATALOG "X"—SENT FREE!

Satisfaction Guaranteed

EDMUND SALVAGE COMPANY, P. O. Audubon, N. J.

THE SCIENTIFIC MONTHLY

VOL. LXIV *June 1947* NO. 6

EXPLORATION OF THE UPPER ATMOSPHERE BY MEANS OF ROCKETS

By H. E. NEWELL, JR.

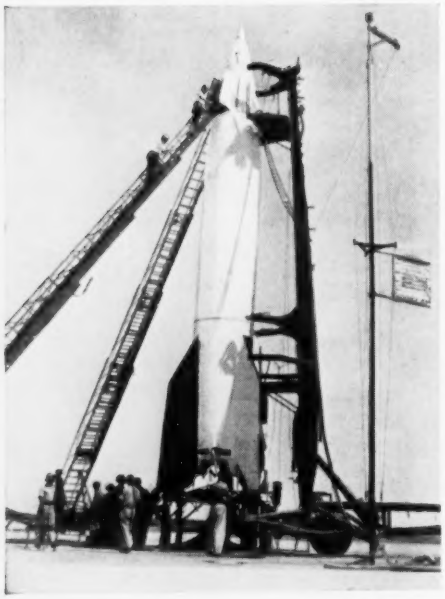
Naval Research Laboratory, Office of Naval Research, Washington, D. C.

AT THE present time the upper regions of the earth's atmosphere are of special interest to scientist and layman alike. For the first time in history the portions of the atmosphere above the 32 kilometers (19.8 miles) attained by balloons are accessible to direct observation. Rockets and rocket-borne instruments already have reached altitudes of 175 km., and it is simply a question of time before even greater heights are a matter of course.

To the scientist, the advent of the rocket as a vehicle for high-altitude research is a welcome thing. In the past a tremendous amount has been learned about the upper atmosphere above 30 km., but only by inference from a wide variety of observations made from outside the region under investigation. By piecing together information from studies of meteors, the aurora, the propagation of sound and radio waves, and other phenomena, it has been possible to obtain a fairly good picture of the state of the atmosphere at least up to 120 km. There are, however, many questions that remain unanswered. Now, by the use of rocket-borne instruments, it is possible to provide some of the answers.

The rocket does not, of course, displace other methods of investigating the upper atmosphere. It merely supplements them. The principal advantage of the rocket is that it actually gets into the region under study. But there are also certain obvious shortcomings. First, at least for the present, a rocket flight is relatively short. The longest V-2 flight to date took only between nine and eleven minutes. Thus the period of time available during a flight for the making of measurements is limited. Also, there are definite limitations imposed upon the sizes and weights of experimental equipment. The performance of the vehicle itself, vibration, rolling, and supersonic speeds introduce experimental difficulties. Experiments requiring observations over an extended period of time for the determination of such things as diurnal and seasonal effects are at present ruled out. In spite of such limitations, however, the rocket can provide a valuable service in high-altitude research. In fact, a good beginning has already been made in the use of the V-2 for this very purpose.

For the past several months German V-2 missiles have been employed in an intensive program of upper-atmosphere research. The



Naval Research Laboratory Photo

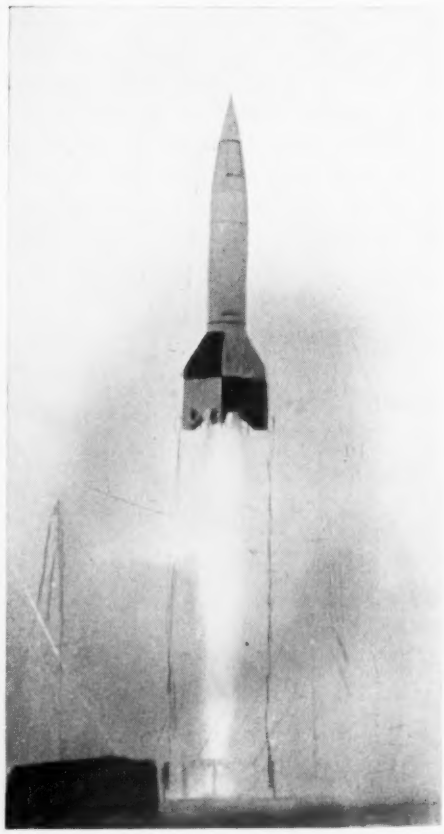
FIG. 1. V-2 ON LAUNCHING STAND

missiles were captured in Germany at the close of the war in Europe and are now being fired at White Sands in New Mexico by the Army Ordnance Department.

The high-altitude research is actually one phase of a three-purpose program: to obtain experience in the handling of large rockets; to obtain ballistic and aspect data for the missile; and to perform upper-atmosphere experiments. The first phase is being conducted by the General Electric Company under contract to Army Ordnance. The ballistic program and general tracking of the missile are being carried out by personnel from the Army's Aberdeen Proving Ground. Several agencies are taking part in the instrumentation of the V-2's for exploring the upper atmosphere. Their efforts are coordinated by a V-2 Upper Atmosphere Panel, the chairman of which is E. H. Krause, of the Naval Research Laboratory. The Panel was formed at the invitation of the Army Ordnance Department some months prior to the first firing at White Sands on April 16, 1946.

The agencies actively participating in the upper-atmosphere program are: the Naval Research Laboratory, Washington, D. C.; The Johns Hopkins University Applied Physics Laboratory, Silver Spring, Md.; Princeton University; and the Army Air Forces through the University of Michigan and the Watson Laboratories in Cambridge, Mass. Princeton University dropped out of the program early in 1947, and the Army Signal Corps has since entered into the work.

The firings at White Sands occur at the rate of one every two weeks. They are grouped by fours into cycles, each agency instrumenting one missile per cycle. Seventeen missiles were launched in 1946. Up to



Naval Research Laboratory Photo

FIG. 2. V-2 TAKES OFF

March 15, 1947, a total of 21 had been fired, and it is expected that the final total will be at least 50 V-2's fired. At the current rate of firing every two weeks, this means that the V-2 program will extend at least into the summer of 1948.

So far the Naval Research Laboratory has provided the upper-atmosphere research equipment for three V-2 flights, the most successful of which occurred on October 10, 1946. Figure 1 shows the October 10 rocket as it rested on the launching platform prior to the firing. The missile stood a full 46 ft. 11 in. above the top of the platform. Its diameter was 5 ft. 5 in. Some idea of its huge size can be obtained by comparison with the men in the picture.

The second figure shows one of the earlier rockets just after take-off. The thrill which the sight of such a launching brings can hardly be expressed in words. At the moment of take-off the jet exerts just enough force to lift the missile, which rises gently and gracefully. The full thrust of 28.3 tons is turned on the moment the missile starts to move, however, and within a few seconds the speed of the missile exceeds that of sound. On October 10, at the time of fuel burnout, the rocket was at an altitude of 25 miles, or 40 km., and was moving with a speed of nearly a mile per second.

Some of the principal features of the October 10 flight are shown graphically in Figure 3. Plots of altitude and velocity versus time appear on the right, and the trajectory itself is given on the left. The horizontal motion of the rocket was small in comparison with the 175-kilometer height attained. The maximum altitude was reached at 238 seconds after take-off. The shower effect was drawn in at the end of the trajectory to indicate that the missile broke up after about 410 seconds of flight.

The air burst was caused intentionally by exploding charges placed just behind the warhead. Two methods for detonating the explosive were employed. One used an

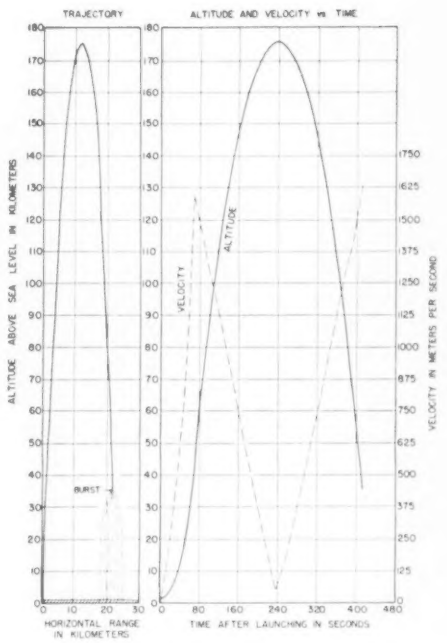
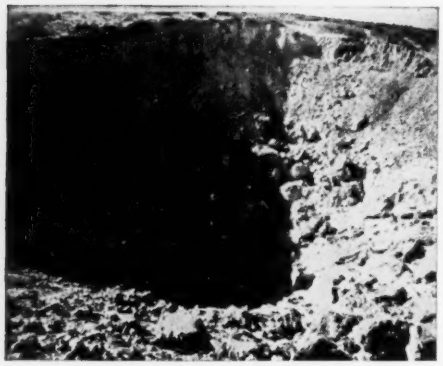


FIG. 3. V-2 TRAJECTORY, OCT. 10

automatic timer set to operate at 330 seconds after take-off. The other used a remote-control device, the radio signal for which was transmitted at 335 seconds after take-off.

Parts of the rocket were strewn over an area extending from 19 to 27 km. from the launching site. There is, accordingly, no doubt that an air burst occurred. Since, however, radio signals were received from the missile for 410 seconds, it appears that the explosive merely weakened the missile structure at some time between 330 and 335 seconds after take-off, and complete disintegration occurred some 75 seconds later. At this time, as the graphs of Figure 3 show, the missile's altitude was 35 km., and its speed was 1,640 meters per second, more than 1 mile per second!

The air burst was caused to facilitate recovery of equipment from the missile. On previous flights in which the rocket struck the earth intact, tremendous craters were formed. One such crater is shown in



Naval Research Laboratory Photo

FIG. 4. CRATER FROM V-2 IMPACT

Figure 4. That resulting from the impact of the June 28 rocket measured 80 feet across and was more than 30 feet deep. In spite of an intensive search involving more than a week of excavating the crater and sifting through the sand, nothing of value was recovered. On the other hand, the afterbody of the July 30 missile, for which the attempt to cause a breakup in midair was successful, was only slightly damaged. Attached equipment, if appropriately protected, could have been recovered. Figure 5 shows portions of the October 10 rocket which were found after the flight.

A schematic diagram of the instrumentation of the October 10 V-2 appears in Figure 6. The seeds, none of which were recovered after the flight, were provided by Harvard University for the purpose of determining the effect, if any, of cosmic radiation at high altitudes in producing mutations. The cameras were installed primarily to study the motions of the trailing wire antennas which were attached to the fins. Five of the cameras mounted on the venturi ring shown in Figure 5 were recovered with the films undamaged. The spectrograph was moved from the warhead location used on June 28 to the fin position shown in the figure, because it was felt that the change would increase the likelihood of recovering the instrument after the flight. The warhead was designed at the Naval Research Laboratory specifically for use in upper-atmosphere research and was constructed at the Naval Gun Factory in Washington, D. C. The explosive for causing an air burst is shown installed in the control compartment just behind the warhead. Also in the control compartment was the telemetering equipment used for relaying data to the ground by radio.

The Naval Research Laboratory telemetering equipment has been used on all V-2 flights at White Sands. The airborne unit, when in operation, continuously transmits groups of one-microsecond pulses. Each group consists of an initial pulse followed by a sequence of 23 pulses. The last

Naval Research Laboratory Photo

FIG. 5. PORTIONS OF THE V-2 RECOVERED AFTER THE OCTOBER 10 FLIGHT

pulse of a group is always separated from the initial pulse of the next group by a very much longer time interval than ever exists between the pulses within the group itself. In this manner the different groups of pulses are clearly distinguished. The groups are transmitted at the rate of 180 per second, the time interval between the initial pulse and that of the next group always being exactly 0.00555 second.

The time spacings of the pulses within a group correspond to the various channels of the system. Thus, the spacing between the initial pulse and the second pulse may be said to correspond to channel No. 1 of the system; that between the second and third pulses, to channel No. 2; etc.

If, when a group of pulses is formed for radio transmission to the ground, a d-c voltage V_1 exists at the input to channel No. 1, the spacing between the first and second pulses is $50 + 30\ V_1$ microseconds. Similarly, if V_n exists at the input to channel No. n, the spacing between the nth and the $(n+1)$th pulses is $50 + 30\ V_n$ microseconds. Thus, the spacings between pulses within a group furnish a measure of the voltages existing at the inputs to the various channels at the time of transmission of the group.

It is plain that if the voltages at the inputs of the channels are themselves measures of physical quantities, then radio transmission of the appropriately spaced pulses described above is a means of measuring those physical quantities from a distance.

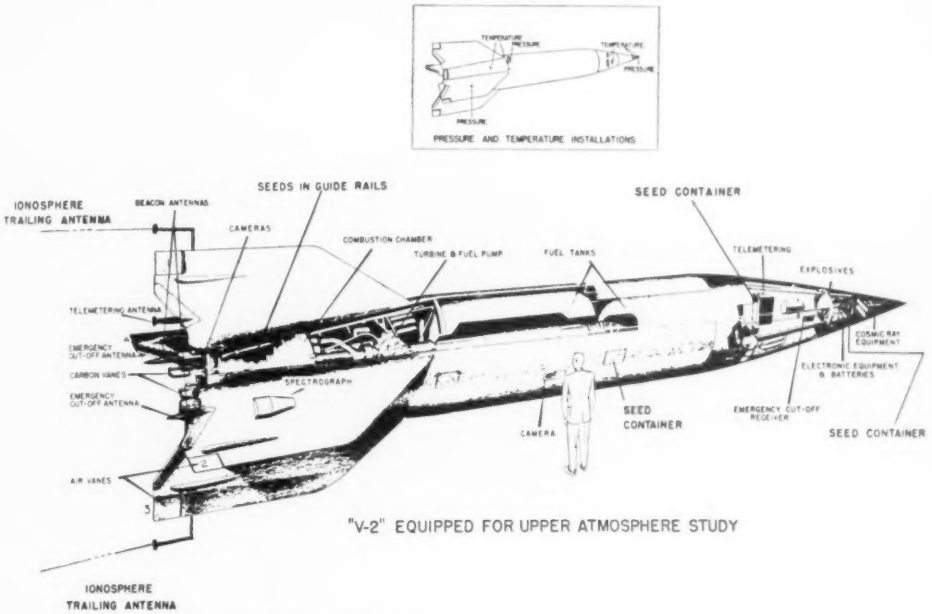


FIG. 6. SCHEMATIC DIAGRAM OF V-2 INSTRUMENTATION

The ground stations used at White Sands contain equipment for receiving the pulses transmitted from the rocket and for translating the pulse spacings back into the original voltages. The recovered voltages may then be applied to a recording oscillograph which provides a photographic record of them suitably separated into channels for subsequent analysis.

For example, in the October 10 rocket, a bellows gauge was coupled to a potentiometer which controlled the magnitude of the

FIG. 7. TELEMETERING RECORD

ARROWS INDICATE RISING VOLTAGE CORRESPONDING TO DECREASING PRESSURE IN BELLOWS GAUGE.

voltage applied to a specific channel. One end of the range of pressures to be measured was made to correspond to a zero voltage input to the telemetering channel, and the other end to 5 volts d-c, these limits being chosen since all voltages applied to telemetering channels were required to lie between 0 and 5 volts d-c. A calibration curve of voltage versus pressure was obtained for the gauge before the flight and was used after the flight in the interpretation of the telemetering record. A portion of the telemetering record obtained for the gauge on the October 10 flight is shown in Figure 7.

The experiments which used telemetering for obtaining measurements were those on cosmic rays and on pressures and temperatures. In addition, various quantities connected with the rocket performance, such as motion of the carbon vanes used to direct the jet and thereby the missile, were telemetered.

The most exciting record from the October 10 flight, however, was not obtained by telemetering. The solar spectroscopy experiment depended upon recovery of a photographic film for getting data. Both the spectrograph and the film were recovered after the October 10 firing. Subsequent development of the film revealed the first spectrograms ever obtained from above the ozone layer in the earth's atmosphere.

A schematic drawing of the spectrograph which on October 10 made its historic journey into the upper atmosphere is shown in Figure 8. The diagram is to a large extent self-explanatory. As shown, the illuminators are small beads. The purpose in using bead illuminators was, of course, to obtain a wide-angle system to eliminate the need for tracking the sun, and tests showed that the use of such beads provided a greater illumination, requiring shorter film-exposure times, than did a conventional slit of comparable width together with a diffuser plate. The beads were 2 mm. in diameter and were made of carefully polished lithium fluoride, which is the most transparent substance known for ultraviolet light. The grating was ruled at The Johns Hopkins University. It was 4 cm. long by 3 cm. high and consisted of 15,000 lines to the inch ruled on an aluminized spherical surface of 40 cm. radius of curvature.

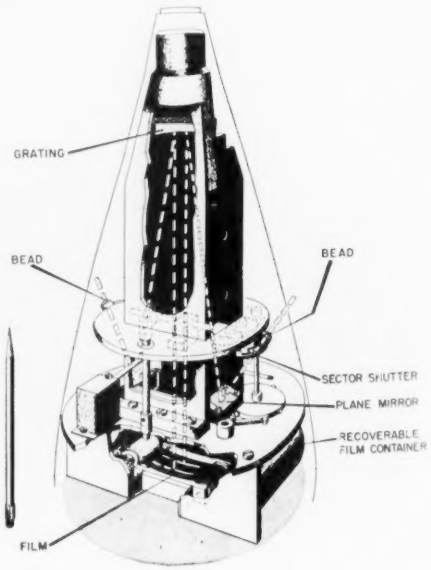
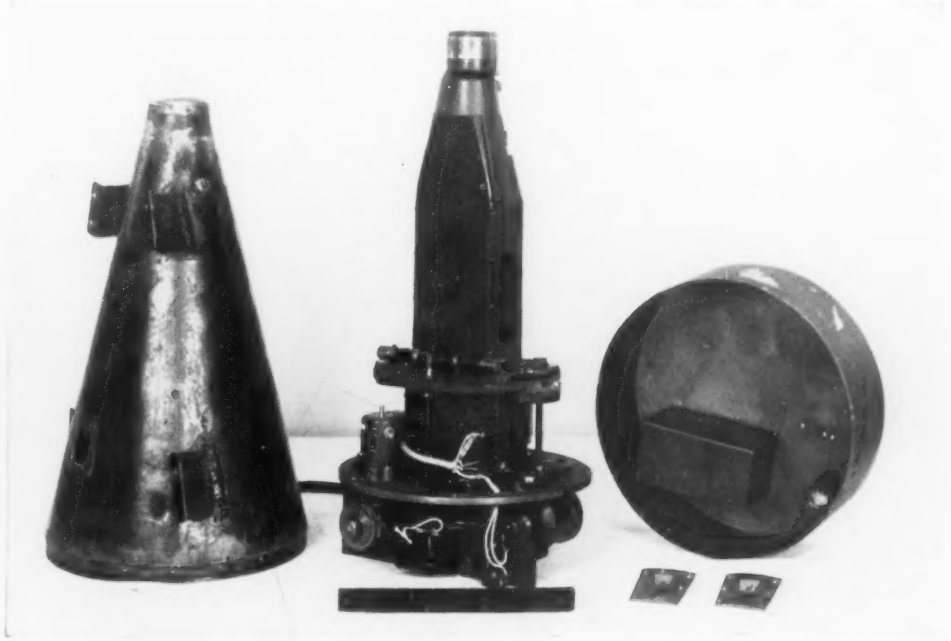


FIG. 8. SPECTROGRAPH, SCHEMATIC

Eastman 35-mm. 103-0 ultraviolet sensitized film recorded the spectra.

The purpose in using two beads was in effect to combine two spectrographs into one. The instrument was installed in Fin 2 of the rocket so that at the time of launching one bead looked south and the other looked north. Both beads were directed forward at an angle of 45° to the longitudinal axis of the rocket, and each was capable of directing the sun's light into the spectrograph even when the sun lay as far as 70° off the principal axis of the system. Since the rocket rolled after fuel burnout, when the stabilizing effect of the jet vanes was removed, there were periods during which the spectrograph film was insufficiently illuminated to produce useful spectrograms. The use of two beads in a single instrument considerably reduced the amount of wasted time. To avoid smearing of the spectrograms, the light from one bead was directed to one edge of the film, and that from the other bead was directed to the other edge.

The spectrograph itself, which was constructed by Baird Associates according to the Naval Research Laboratory design, is shown in Figure 9. This might easily be taken as a "before" picture. Actually, the photograph was taken after the instrument had been returned to the Naval Research Laboratory, following its recovery from the missile wreckage. Two small dents can be seen in the housing on the left in the figure. Except for slight pitting of the illuminator beads, presumably due to weathering in the desert, this is the only damage suffered by the spectrograph. Advantage is being taken of the excellent condition of the recovered instrument to run new calibrations on it in addition to those made before the flight.



Naval Research Laboratory Photo

FIG. 9. SPECTROGRAPH RECOVERED AFTER OCTOBER 10 FLIGHT

Figure 10, which was published originally in *The Physical Review*,[1] shows some of the spectrograms obtained on October 10.

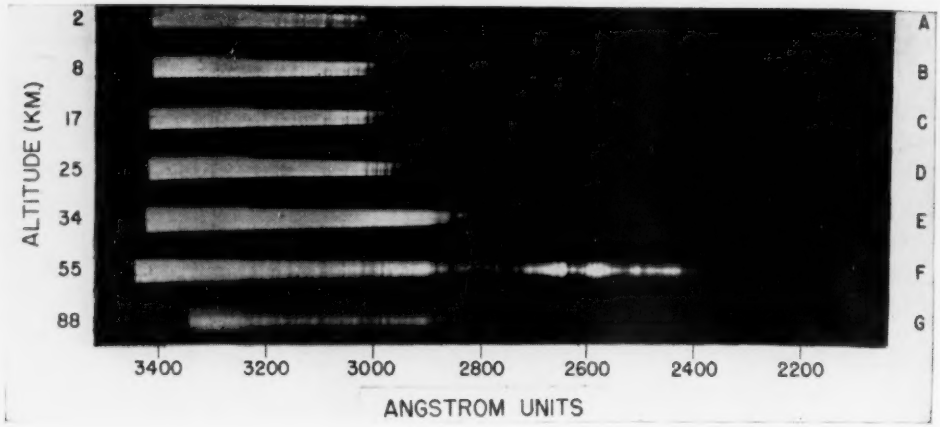


FIG. 10. SPECTROGRAMS OBTAINED ON OCTOBER 10

The altitudes at which they were taken are shown at the left. One observes a progressive extension of the spectrum into the shorter wave lengths as altitude increases. At 34 km. the ozone absorption is still sufficiently strong to block out the wave lengths in the central region of the Hartley bands on either side of 2,550 A. Some radiation, however, does come through in the region below 2,200 A. On the other hand, it appears that very little ozone remains above 55 km. The spectrogram taken at that altitude shows a strong influx of radiation in the central portions of the Hartley region. By the time the rocket had reached 88 km., the sun was well off the optical axis of the spectrograph, and there was insufficient illumination at the shorter wave lengths to photograph. This explains the faintness of the spectrogram taken at 88 km. Malperformance of the spectrograph prevented the taking of spectrograms to the highest altitude attained by the rocket.

The taking of solar spectrograms from above the ozone was not the only "first" associated with the October 10 rocket. Cosmic-ray data obtained in that flight provided the material to complete the well-known Pfotzer curve of vertical intensity of cosmic radiation versus altitude, or, as it is usually plotted, versus atmospheric pressure. The first reliable determination of the point on that curve corresponding to zero pressure was made. Zero pressure was taken in the reduction of data to mean all pressures below 2 mm. of mercury, which in terms of altitude means any height in excess of approximately 40 km.

A schematic drawing of the counter array used in the cosmic-ray experiment appears in the upper right-hand corner of Figure 11, which was first published in *The Physical Review*.[2] The numbered circles denote Geiger counters arranged in the form of a multiple telescope which, as indicated, looked out at 45° to the vertical through a thin steel window.

The information telemetered during the flight was: (*a*) the coincidences $1 \cdot 2 \cdot 3 \cdot \bar{4} \cdot 5$; (*b*) the coincidences $1 \cdot 2 \cdot 3 \cdot \bar{4} \cdot 5 \cdot (6 + 7 + 8)$; (*c*) the coincidences $1 \cdot 2 \cdot 3 \cdot \bar{4} \cdot 5 \cdot 6 \cdot 7 \cdot 8$; and (*d*) the coincidences $1 \cdot 2 \cdot 3$. The notation used here is analogous to that of point set theory. The dot can be read as "and," the plus sign as "at least one of," and the superscript bar as "not." Thus a coincidence $1 \cdot 2 \cdot 3 \cdot \bar{4} \cdot 5 \cdot (6 + 7 + 8)$ means a count simultaneously in counters 1 and 2 and 3 and at least one of 6, 7, and 8, but not in 4 and not in 5. Actually, the word "simultaneously," as used above, does not have the meaning "at one and the same time."

Rather, counts in a group of counters are considered to be simultaneous if they occur within such a small time interval of each other that the probability of their being caused by separate or unrelated particles is very remote. For the counting rates to be expected in the upper atmosphere it is safe to regard counts which occur within about 20 microseconds of each other as coincidences. The coincidence resolution employed in the October 10 experiment was about 5 microseconds.

A coincidence $1\cdot2\cdot3\cdot\bar{4}\cdot5$ must be caused by a ray or shower coming from a small solid angle about the direction of the line of centers of counters 1, 2, and 3. The recording of counters 4 and 5 in anticoincidence with 1, 2, and 3, that is, the requirement that 4 and 5 do not fire when 1, 2, and 3 do, eliminates all coincidences $1\cdot2\cdot3$ due to showers of particles entering from the sides of the telescope. Thus the number of coincidences $1\cdot2\cdot3\cdot\bar{4}\cdot5$ per unit time divided by the solid angle of the telescope gives the total cosmic radiation intensity from the direction of the steel window.

In a similar fashion, the coincidences $1\cdot2\cdot3\cdot\bar{4}\cdot\bar{5}\cdot(6+7+8)$ per unit time can be used to determine the intensity of the hard, that is, the penetrating or highly energetic, radiation from the direction of the window. Such a coincidence can be caused only by a particle or shower of sufficient energy either to penetrate 15.2 cm. of lead or to set off some sequence which persists through that much lead. In all this discussion the term "particle" is taken to include photons as well as electrons, protons, mesons, etc.

The coincidences $1\cdot2\cdot3\cdot\bar{4}\cdot\bar{5}\cdot6\cdot7\cdot8$ provide a measure of the hard shower count. Since in this case all of 6, 7, and 8 must fire in coincidence with 1, 2, and 3, a shower of at least 3 particles must emerge at the bottom of the lead.

Finally, the coincidences $1\cdot2\cdot3$ per unit time diminished by the coincidences $1\cdot2\cdot3\cdot\bar{4}\cdot\bar{5}$ per unit time is a measure of the total shower rate.

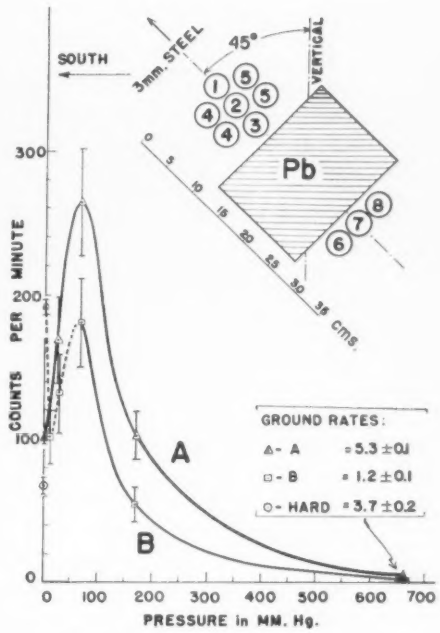


FIG. 11. COSMIC RAY INTENSITY

The curves in the lower left-hand corner of Figure 11 show the results obtained on October 10 by measuring the coincidence rates discussed above. Curve *A* gives the measured total intensity of cosmic radiation from a 45° angle to the vertical. This is the curve which corresponds to the Pfotzer curve mentioned earlier. The Pfotzer curve, which gives vertical radiation intensity versus atmospheric pressure, has until now lacked the portion corresponding to altitudes above those attainable by balloons. In the previously known portions, the correspondence between *A* and the Pfotzer curve is good if appropriate account is taken of the fact that the two curves refer to different directions relative to the vertical. It follows that the free space point of *A* serves in effect to complete the Pfotzer curve. In fact, if, as seems likely, the cosmic radiation in free space is essentially isotropic, then *A* and the Pfotzer curve should come together at the zero pressure point.

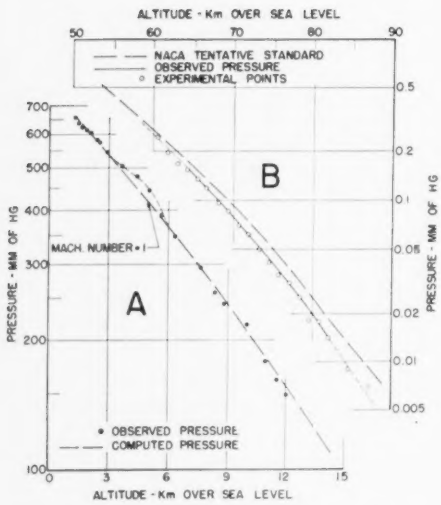


FIG. 12. PRESSURE CURVES, OCT. 10

The circle on the zero pressure axis gives the hard count in free space. The solid angle of the hard-count telescope was only .37 times that of the total-count telescope, and there were not enough counts at the altitudes below 2 mm. of mercury to provide reliable data. If it be desired, the ground point, determined by careful calibration prior to the rocket flight, can be used in conjunction with the curve already known for the altitudes reached by balloons, to fill in the hard-count curve. As can be seen, only about 70 percent of the radiation in free space is penetrating.

Finally, Curve *B* is a measure of the shower count. It was obtained as described above by subtracting the counts $1 \cdot 2 \cdot 3 \cdot \bar{4} \cdot \bar{5}$ from the counts $1 \cdot 2 \cdot 3$. The dip at 12 mm. of mercury is felt to be questionable. The high shower rate in free space appears to be reliable, however, and is surprising. Presumably the showers are formed in the warhead. But how? That is the question.

The curves of Figure 12, which also was published originally in *The Physical Review*,[3] show some of the data obtained on pressures in the upper atmosphere. The measurements were made by means of gauges installed in the tail of the October 10 rocket at points where German wind-tunnel data indicated ambient pressures would be measured.

The circles of Curve *A* denote pressures recorded by means of a sylphon bellows gauge, the telemetering record for which was shown in Figure 7. The broken-line curve was computed from balloon sounding data taken at El Paso on the morning of the flight. The fact that the observed and computed pressures agreed so well outside the ever-troublesome region of Mach number 1, when the missile's speed was in the neighborhood of that of sound, indicated that at the location of the bellows gauge the pressures were actually ambient pressures.

It was therefore assumed that the readings of a Pirani gauge similarly located in the tail of the V-2 gave ambient pressures directly. These observed pressures are plotted as the circles in *B*. The broken-line curve is the tentative standard of the National Advisory Committee for Aeronautics, inserted for comparison purposes. It is seen that the measured points exhibit a curvature similar to that of the NACA standard. This curvature corresponds to a negative temperature gradient. The measurements may, therefore, be taken as partial confirmation of the existence of an atmospheric temperature which decreases with altitude in the range from 60 to 80 km.

The inflection in the NACA curve just beyond 80 km. appears to exist in the curve of observed pressures, but there are in this region too few measured pressures from which to draw any firm conclusions.

The October 10 rocket also carried equipment for measuring ion density in the ionosphere. As it happened, however, the loss of one of the trailing wire antennas at launching prevented successful performance of the experiment.

Other experiments, installations, and measuring equipment were included in the October 10 V-2 instrumentation. Temperature measurements were made in addition

to those of pressure. A timer was included to detonate the explosive for separating the warhead from the afterpart of the rocket. Auxiliary measurements were made for the various experiments. For example, the motion of the spectrograph film was telemetered to provide a means for determining the altitudes at which the various spectrograms were taken. A Naval Research Laboratory ejection device, for the recovery of special equipment and records, was tested during the October 10 flight. The Army also included an ejection device of its own.

It is interesting, and perhaps ironic, that the only things of any real importance that were not recovered from the October 10 flight were the recovery devices. All evidence points to the fact that the ejection devices did operate properly, but, since the containers were ejected long before the missile broke up, they presumably do not lie in the area over which the missile wreckage was strewn. Thus, the failure of the ejection experiments appears to be due to the difficulty of locating the containers after they have landed, rather than in getting them safely to earth. One can readily think of many schemes for overcoming that difficulty, but for the present the ejection experiments are being curtailed owing to the highly gratifying success so far attendant upon the use of the airburst method.

A highly detailed and technical account of the Naval Research Laboratory's participation in the V-2 program appears in two specially prepared reports.[4, 5] These cover the Laboratory's accomplishments in upper-atmosphere research through the October 10 firing.

It is plain that the performance of as many experiments as described above, all at once, and in the same rocket, requires considerable manpower and very careful coordination of effort. The Naval Research Laboratory has at present a large group of professional men engaged solely in upper-atmosphere studies. E. H. Krause directs the Laboratory's participation in high-altitude research, a major part of which at the present time is associated with the V-2 program. For the October 10 flight G. J. Perlow directed the cosmic-ray work. C. V. Strain and R. Tousey maintained cooperative direction of the experiment in solar spectroscopy. The temperature and pressure measurements were conducted by N. R. Best and R. J. Havens. The work on the ionosphere experiment is progressing under T. R. Burnight. C. H. Smith is in charge of the electronic instrumentation and rocket services. He is assisted by M. W. Rosen and J. T. Mengel. The author directs the work of a theoretical group.

All this, however, is not the whole story. To get a complete picture of the effort being put forth on the upper-atmosphere program, one must count, in addition to the men listed above, shop personnel, machinists, sheet-metal workers, glass blowers, draftsmen, and outside contractors. And to these must be added those engaged in assembling, servicing, and firing the rockets, and those engaged in tracking and photographing the missiles during flight.

REFERENCES

1. Baum, W. A., Johnson, F. S., Oberly, J. J., Rockwood, C. C., Strain, C. V., and Tousey, R. Solar Ultraviolet Spectrum to 88 Kilometers. *Phys. Rev.*, November 1946, **70**, 781.
2. Golian, S. E., Krause, E. H., and Perlow, G. J. Additional Cosmic-Ray Measurements with the V-2 Rocket. *Phys. Rev.*, November 1946, **70**, 777.
3. Best, N. R., Durand, E., Gale, D. I., and Havens, R. J. Pressure and Temperature Measurements in the Upper Atmosphere. *Phys. Rev.*, December 1946, **70**.
4. Upper Atmosphere Report No. I. *Naval Research Laboratory Report R-2955*, October 1, 1946.
5. Upper Atmosphere Report No. II. *Naval Research Laboratory Report R-3030*, December 30, 1946.

A LAYMAN LOOKS AT SCIENCE

By JOHN L. CHAMBLISS

Falls Church, Va.

NOT long ago my young son, who is in the sixth grade, came home from school with the announcement that he had learned about the origin of the earth. "There used to be just the sun," he explained, "and then a star came along and came too close to it, and by the force of gravitation it drew out a sort of streamer that cooled and made the planets and the earth."

I said that was the theory, though of course it hadn't been proven.

"That's why all the planets are going around the sun."

I said yes. "But don't forget it's just a theory. It's what the scientists call a hypothesis."

My words, I am afraid, made little impression on him. He had learned something new—how the earth had originated—and the distinction between "theory" and "truth" was too much for him to make.

I came across the following in a newspaper editorial recently, written on the occasion of the quadricentennial of Tycho Brahe: "He was the pioneer in the affirmation of the supreme value of data. The impact of his career upon the existence of people now living cannot be exaggerated. He was, indeed, one of the principal architects of the world of today."

Though one may be pardoned for thinking that the great Danish astronomer might find the last remark a rather dubious compliment, there is no denying that science considers itself, and justly, as the principal architect of the twentieth-century world, and by this is usually meant that the material conditions of life today are directly or indirectly traceable to science.

There is, however, a more fundamental way in which science is the "architect" of our world: what might be called its subjective creation as distinguished from its objective creations in the world around us. For the layman who chooses to think about such things, it is, perhaps, more important than the visible evidences of everyday life. I refer to that *world-picture* which science has given to modern man, his "cosmological environment" which is the result of the discoveries of the past four centuries.

When my eleven-year-old son learned in school that the earth originated in such and such a way (and because of his years failed to distinguish between "truth" and "hypothesis"), he was adding all unknowingly to his "cosmological environment"—that picture of the universe which he will carry the rest of his life.

It may seem an impertinence for a layman to express himself on scientific matters, even those of a general nature which come home to him most closely. Science in recent years has taken on almost the status of a priestcraft in the minds of many laymen: we are made aware that its discoveries, which overshadow our lives, are comprehensible only by the few.

On the other hand, every man has the right to philosophize on general questions, and by "general questions" I mean that picture of the world and life and God which is the foundation of our very existence.

That a man is conditioned by his environment we all know, and it should be equally plain that men in general are affected by their larger environment, that "cosmological environment" in which they share as partakers of the same civilization. It should be obvious, I think, to any reasonable man that if the men of any age picture themselves

as living in a universe in which they do not really belong, in which their God is an abstraction, their earth the result of a cosmic accident, and they themselves and life itself but fortuitous phenomena, having no real status in the universe as a whole: if this be their world-picture, it is bound to be reflected in their lives and in their civilization.

Man is distinguished from the lower animals by the fact that he knows he is a Man, and it is this self-consciousness which, in a sense, *makes* him a Man. The first Man was he, perhaps, who first made this discovery, and by that act *became* a Man, and it is worth remarking that the tribal name of primitive peoples is simply "Men."

But the corollary of this self-consciousness, which has lifted a good portion of mankind from savagery to civilization, is man's belief in his own worth and dignity. Destroy that belief, convince men in general that they and all their works are of no ultimate consequence, and though man may continue to exist as a higher animal, he will in time return to savagery. For we may lay it down as a general proposition, I think, that the first necessity of creative man, individually and collectively, is a belief in his own importance.

Among scientific writers of the past thirty years or so it has been the fashion to stress the vast extent of the universe, the insignificance of the earth and the solar system in this scheme of things, and the corresponding insignificance of man. Scientists, it would seem, have conceived it as their duty to inform the rest of us that the human race is not nearly so important as men have supposed in the past, and that even life—which for us is the basis of all perception and thought—is but a sort of by-product in the greater universe.

It may be that good intentions lie behind these teachings, and the scientist would doubtless say that he is merely presenting the world as it is; on the other hand, the human urge to dramatize probably has something to do with it, nor should we forget that, in painting this picture, the scientist has recourse to theory as well as fact, while over it all lies his own personal interpretation.

I should like to illustrate this difference between fact and interpretation, since it lies at the heart of any discussion of our modern world-picture. I quote from a book published twenty years ago but still on the shelves of many libraries, Bertrand Russell's *Philosophy* (chap. ix, The Structure of the Atom):

> The main point for the philosopher in the modern theory is the disappearance of matter as a "thing." It has been replaced by emanations from a locality —the sort of influences that characterise haunted rooms in ghost stories. As we shall see . . . , the theory of relativity leads to a similar destruction of the solidity of matter, by a different line of argument. All sorts of events happen in the physical world, but tables and chairs, the sun and moon, and even our daily bread, have become pale abstractions, mere laws exhibited in the successions of events which radiate from certain regions.

We have here, it will be seen, certain statements and certain inferences drawn from those statements. According to the theory of modern physics (now abundantly verified) the atom is of such and such a nature: a solar system in miniature, we will say, whose "sun" and "planets" are mere radiation points. Very well, says the layman; such is the nature of the atom. Now let us follow our scientist-philosopher as he proceeds to develop this theme.

Matter, we are told, has disappeared, as a "thing." It has been replaced by "emanations from a locality—the sort of influences that characterise haunted rooms in ghost stories." And further on we read that "tables and chairs, the sun and moon, and even our daily bread, have become pale abstractions," and so on.

It will be said that these are mere figures of speech. Nonetheless, let us analyze the meaning behind these statements, which are not untypical of the sort of thing the lay-

man reads today and which are instrumental in fashioning his world-picture.

Matter, we read, has disappeared, as a "thing." We ask therefore what constitutes a *thing*, in the eyes of the scientist. Is a "thing" any less a "thing" because we have learned that its ultimate constituent is a system of energy?

What did the scientist expect to find when he started out to investigate the properties of matter? Surely he did not expect to find that a piece of bread was always a piece of bread. Or that a chair was always a chair. Why, then, having discovered that the fabric of the universe is a *unity* which we call energy, does he assume that the visible world is any less "real" than it was before?

Plainly, we are dealing here with something more than the bare data of science. Nor can the scientist, who thus adds his own personal interpretation to his data, honestly claim to be presenting the world as it is. What science has proven in the past thirty years, it would seem to a layman, is that the visible world of multiplicity is composed of an invisible world of unity, and is itself the *form* of that invisible: a discovery foreshadowed two thousand years ago in the Greek philosophers. But this does not imply that the visible world is any less "real" than it was before; nor does it mean that we are all characters in a ghost story.

These features are the addition of the individual scientist in interpreting his data —an interpretation not uncharacteristic of the way present-day science explains the world to the layman.

It will thus be seen that our modern world-picture is only in part the result of scientific data, that it depends also on the way those data are interpreted. And it is this act of interpretation with which I am concerned.

The unscientific layman who would accept as a matter of course any number of scientific discoveries is not necessarily bound to accept with equal faith the scientific interpretation of those discoveries. We may ask, indeed, whether there is any such thing as "scientific interpretation." One might as well speak of "scientific appreciation" or "scientific enthusiasm." Interpretation, one supposes, is largely a matter for the individual mind, unless we allow the authority of religion or philosophy.

Modern science, however, has long since abandoned religion as a means of interpreting its data, and it would seem that even philosophy has been put aside, if we mean by "philosophy" *common sense and genuine seriousness*—qualities not notably present in the quotation given above. Instead, science has pinned its faith to "the supreme value of data," with the result that today —although the storerooms of science are bulging with facts—the layman has difficulty finding a philosophical scientist who can adequately interpret those facts.

One thinks of Arthur Eddington and Sir James Jeans in this connection, along with other noted scientists who have addressed themselves to this task, but it seems to me that even they fail to appreciate the *seriousness* of what they are doing: like Bertrand Russell, they like to dramatize their material and, if possible, throw a scare into the reader.

They tell us, in substance (as we all know), that the universe is apparently the work of a Great Mathematician, for whom, it would seem, the earth and life and mankind are matters of the extremest inconsequence. They give us, in other words, a world-picture the very opposite of that postulated by any religion, wherein the importance of mankind in the eyes of the Creator is a primary requisite.

It follows naturally, if we accept their scheme of things, that there is no argument for virtue but expediency, no meaning whatever in such words as "love," "honor," and "sacrifice;" and even science is left with

little on which to stand, since there is no reason for supposing that in such a cosmos man can get even a glimpse of the truth.

These things, of course, we cannot accept; even the scientist balks at drawing these inevitable inferences, and so it is precisely at this point that he bows out of the picture, as a scientist, leaving the layman to work out his own salvation.

We have, therefore, as an essential feature of the twentieth-century mind, a new type of *dualism*. For on the one hand we have this scientific world-picture, wherein man and all his works are of no final significance; and on the other hand, varying with each individual, we get a personal compensatory creed, which may range all the way from humanitarianism to dogmatic revealed religion. And this new dualism is no less a characteristic of the scientist than it is of the layman. No less than the layman, the scientist must have something to believe in, over and above this world which he explores; he may be an atheist, but even as an atheist he is forced to believe in the essential worth of his own quest of the truth.

With all this, it would seem to any reasonable, intelligent man, there is something fundamentally wrong.

The world is not divided into two compartments, Science and Faith, or whatever we choose to call them; and if it *is* so divided—or, rather, if that is what we believe—then it is time we realized it.

We are told today, by both scientists and religious leaders, that there is no "real conflict" between science and religion, because each has "its own field." The truth is rather that there has never been a greater conflict between the world-picture of science and the teachings of religion; only they are now so far apart that there is no longer any contest.

If this scientific world-picture were solely a matter of incontrovertible fact, there would be no choice for the rational layman but to accept it unreservedly. But, as we have seen, it is not purely a matter of fact, for it depends also on the way facts are interpreted, which is largely a function of the present scientific outlook.

I will cite one other example: that theory of the origin of the earth mentioned at the beginning, which my young son recently learned in school.

According to this hypothesis, which is known, I believe, as the Tidal Disruptive Theory and which was propounded some years ago by Sir James Jeans as a variation on the Chamberlin-Moulton planetesimal hypothesis, the solar system had its origin in the chance approach of a passing star which, coming too near the sun, drew out from that body a gaseous filament which in time cooled and solidified to form the planets, including our earth.

With the technical aspects of this theory I am wholly incompetent to deal, nor am I interested in them. I am interested in the state of mind behind this conception, whereby the earth and the solar system are conceived as the result of an *accident*.

Here, it would seem, is one of the great problems of science: to account for the origin and the history of the earth. And in this hypothesis by one of the world's most noted scientists, we have, surely, a fair example of today's scientific outlook in its attempt to explain the universe.

The result, as we can see, is a hypothesis in which there is no slightest suggestion that there is a *purpose* in the universe, or that this earth on which we live is the result of any *plan*. Instead, we have a theory of which the scientist asks but one question: Does it conform to known data?

I am not so naive as to expect the hand of God in a scientific theory (though, after all, it is as easy to believe in a miracle as to believe in an accident), but I do expect such a theory to be reasonable and to take some account of other data besides the bare bones of physics and mathematics. And this theory is not reasonable. Taken as one aspect of the

modern world-picture and apart from the limited field of science, it is merely the obverse of the Book of Genesis, carried to the point of absurdity, and is equally unacceptable by any sensible man.

For if we thus account for the origin of the solar system and of this earth we live on, then the idea of a Guiding Intelligence behind the universe goes up in a puff of smoke. Our belief in an order and a reason in the universe likewise goes up in smoke, for we say in effect that *by virtue of this accident, we now possess all the order of the universe.*

Science presupposes an Intelligence in the world; otherwise there could be no science. But Intelligence is not merely Intelligence: Intelligence operates toward an *end.* It cannot be abstracted from the will and made into a thing-in-itself, because it is an instrument of the will. The very concept of Intelligence, whether human or divine, implies a *plan* and a *purpose.*

But in this Tidal Disruptive Theory of Sir James Jeans which is being taught my young son, there is no plan, there is no purpose, there is no meaning and there is no sense. It is purely the attempt of a physicist and mathematician to devise an explanation for the origin of the solar system that will conform to certain scientific data.

I did not set out to deliver a blast against science, for I do not conceive myself as a David to tackle such a Goliath. Moreover, the evidence of every man's debt to science is too apparent and ever-present to be questioned by a sensible person.

But it seems to me that if science, as a *logos* of the world, is to continue to command the respect of the thoughtful layman, it must take a new stock of what is called today "the scientific outlook."

We have come a long way from the days when Tycho Brahe, Bacon, and Galileo, in reaction against Scholasticism, affirmed "the supreme value of data." Science, it would appear, is now endangered by a new scholasticism—the Scholasticism of Empiricism, wherein overspecialization and quibbling over data lead to results no less barren than the "concepts" of the Schoolmen.

The first job of science is *to explain the world to man*—not to give him radios, airplanes, and atomic bombs. And in the long run this explanation must command his inward assent, not merely a blind submission to the omniscience of science.

The layman who takes it upon himself, as a rational human being, to examine the present scientific world-picture in its relation to his personal credo (whatever that credo may be) is led inevitably to the conclusion that, for all its remarkable discoveries, science has yet to work out a reasonable picture of the whole.

Such a picture, of course, must rest upon facts.

But there must be also a philosophy behind those facts, and established principles in the light of which to regard them.

CARTOHYPNOSIS*

By S. W. BOGGS

Department of State, Washington, D. C.

MANY primitive societies are quite unaccustomed to maps. For them, territorial and boundary questions are relatively simple and radically different from those of map-conscious nations. For example, when two tribes in a certain region near the Indian-Afghan frontier find difficulty in agreeing upon a common tribal boundary they sometimes have recourse, as recounted by Colonel A. H. McMahon, to laying down a boundary "by oath." A leading man of one side is prevailed upon to undergo the ordea and is accepted by both sides. Holding the Koran firmly on his bare head, the soles of his feet being bare and cleansed of every particle of his own tribal soil, and having taken every precaution to save his soul from perjury, in a scene full of excited tribesmen he steps out, and the course he follows becomes the unquestioned boundary line. It may unexpectedly diverge widely from both claims, but salient points are sometimes found to be marked by crumbling rock cairns of great age whose existence had long been forgotten. Boundary-makers of many nations wish their tasks were as simple and easy!

Map-conscious people, however, usually accept subconsciously and uncritically the ideas that are suggested to them by maps. In part because maps appear to represent facts pertaining to mother earth herself, veracity and authority are frequently attributed to them beyond their deserts. In what may be called "cartohypnosis," or "hypnotism by cartography," the map user or the audience exhibits a high degree of suggestibility in respect to stimuli aroused by the map and its explanatory text.

Sometimes self-hypnotism and illusion occur quite innocently. And frequently a sort of mass hypnotism is practiced by men who attempt to delude the public. Maps may also be used effectively to dehypnotize people; we should therefore consider what maps may be made, and how they may be used to awaken people to an intelligent understanding of the world and the problems of our times.

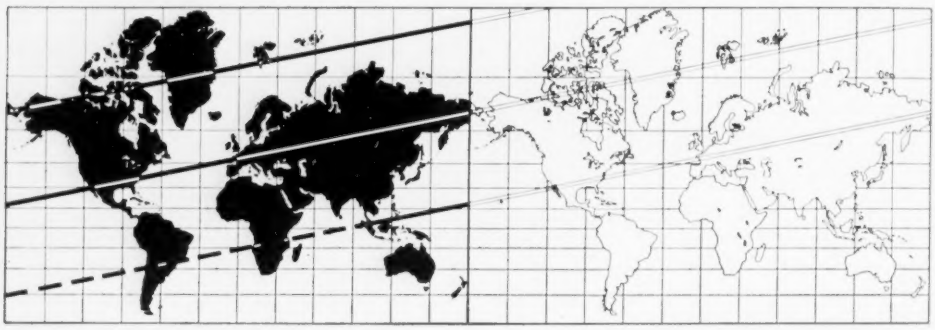
Illusion and confusion. The innocence of some people's illusions when they look at maps uncritically reminds one of a four-year-old child's question: "Why do I see things when I shut my eyes that aren't there when I open them?" People often suppose that maps reveal "facts" which, if they were wide-awake to the maps, they would realize are not shown at all. An example of illusion and confusion, arising from use of the over-familiar Mercator projection, is shown on the accompanying map (Fig. 1*a*) on which there is added a long straight line indicating the true compass course known to mariners as "east by north." On the Mercator map every continuous true compass direction is a straight line,[1] whereas on the earth all such

* From a paper presented at the Fourth Assembly of the Pan American Institute of Geography and History, Section II, Caracas, Venezuela, August 29, 1946.

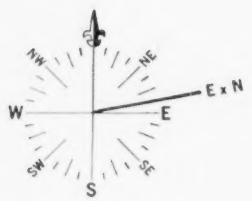
[1] The Mercator map projection is one in which the parallels of latitude, represented by straight lines, are mathematically spaced in such a way that, at any point on the map, the north-south and east-west scales are identical. In consequence, every true compass direction or course is a straight line of indefinite extension; this property makes it especially useful to navigators in plotting courses at sea. The projection is *not* projected onto a cylinder from the center of the globe, as is often depicted—a very different and quite useless map.

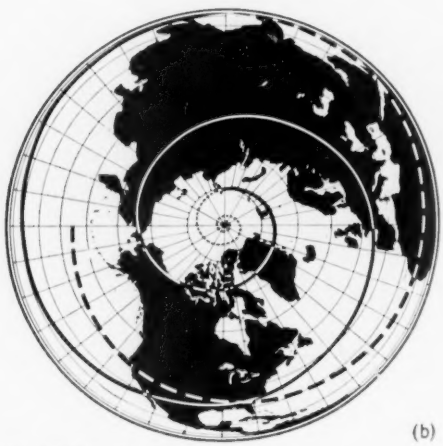
lines are spirals—except the meridians (great circles) and the parallels (small circle). The $E \times N$ line on Figure 1*b* illustrates how such a spiral goes around the earth an infinite number of times without actually reaching the North Pole.

Illusion may occur when men use world maps instead of globes in seeking to understand some of the world relationships of our times. Observe, for example, a world map (Fig. 2*a*) prepared by a brilliant geographer, the late Professor Mackinder (Sir Halford), to illustrate his famous paper on "The Geographical Pivot of History," which he read in 1904. The map was made on the Mercator projection; its limiting border is an ellipse; and parts of North and South America are repeated at the left and right sides of the map. The "pivot area," or "heartland," in north-central Asia (which was for many centuries a region of horse and camel mobility insulated from the continental margins in large degree by deserts) is shown on the map as being bordered by an inner or marginal crescent of land accessible to ships, paralleled by an outer crescent of continents and islands festooned across the map. That



(a)





(b)

FIG. 1. "EAST BY NORTH"

THE COMPASS COURSE KNOWN TO MARINERS AS "EAST BY NORTH" (N. 78° 45′ E.) IS A SPIRAL THAT NEVER QUITE REACHES THE NORTH POLE. ON (*a*), THE MERCATOR MAP, IT IS A STRAIGHT LINE, WHICH WOULD BE CONTINUOUS IF THE MAP WERE REPEATED INDEFINITELY. ON (*b*), THE NORTHERN HEMISPHERE MAP, IT APPEARS AS A TRUE SPIRAL—WHICH IS DOTTED IN ITS POLEWARD EXTENSION BEYOND THE MERCATOR MAP LIMIT. IN THE SOUTHERN HEMISPHERE ON BOTH MAPS THE COMPASS COURSE IS CONTINUED AS A BROKEN OR DASHED LINE.

is how it appears on a particular map. When the Mackinder map is traced on a transparency and wrapped around a cylinder (and the Mercator map is, of course, developed mathematically as on a cylinder), the repeated areas will overlap (Fig. 2*b*). But what is it like on the earth itself? As seen on the globe (approximated by the circular world map, Fig. 2*c*), the left and right portions of the elliptical Mackinder world map correspond to a single lens-shaped area embracing that portion of the Americas which appears twice. To the north and south there are loops enclosing the polar regions which are altogether missing from the map. The remainder of the earth's surface appears on the Mackinder map once and once only. The so-called outer crescent, whose ends overlap in the Americas and which traverses Australia and southern Africa, is seen on the globe as a belt obliquely encircling the earth; also, the Arctic area is seen in its spherical compactness in normal relationships to Eurasia and North America.

Professor Mackinder's own concept of "global realities" was clearly revealed in these words: ". . . We shall . . . have to deal with a closed political system . . . of world-wide scope. Every explosion of social forces . . . will be sharply re-echoed from the far side of the globe." However, it would seem that the Mercator map suggested to its author the concept of an outer crescent, instead of what in reality is approximately an oblique circle; it seems to have suggested, also, an oversimplified generalization in a sort of geometrical pattern of historical relationships. Much of Mackinder's paper, with the salutary critical comment that followed its presentation, is almost forgotten. In any event, the map subsequently exerted a hypnotic influence on many thousands of people, for it was reproduced at least four times in the Nazi literature of so-called geopolitics, with perversions of the author's original intent which were designed to serve malevolent purposes in propaganda.

Delusion by design. Maps are often deliberately employed to "sell" ideas—to individuals and nations. In every continent maps have been used, and are now being used, to disseminate mischievous half-truths and to obfuscate the thinking of men. They are employed as graphic devices—subtly to suggest an idea, to inculcate a prejudice, or to instill patriotic fervor. Such maps may be true in every detail, but in their omissions and their perverse emphases they may be socially poisonous—as chlorine by itself is a poisonous gas but an essential element in common salt.

In an article entitled "Magic Cartography" (*Social Research*, September 1941), relating to the uses of maps in propaganda, Hans Speier observes:

> The use of maps in propaganda is dependent upon highly developed techniques of map making and reproduction, a certain minimum of mass education in reading cartographic symbols and a specific organization of society. This organization may be characterized briefly as one in which the individual's functional dependence and loyalties extend far beyond the area of his immediate experiences.
>
> . . . [Maps] may make certain traits and properties of the world they depict more intelligible—or may distort or deny them . . . . They may give information, but they may also plead. Maps can be symbols of conquest or tokens of revenge, instruments for airing grievances or expressions of pride. Indeed, maps are so widely used in propaganda and for such different purposes that it is difficult to understand why propaganda analysts have paid so little attention to them.
>
> Propagandists . . . rediscover . . . symbolic values in maps, and by exploiting them, turn geography into a kind of magic. . . . The progagandist's primary concern is never the truth of an idea but its successful communication to a public.
>
> Entirely new possibilities in the use of maps for political propaganda are revealed by the *film*. The German propagandists have realized that. . . [when they produced] moving maps.
>
> . . . [Maps] are essentially scientific. The propagandist who uses them borrows the prestige of science and at the same time violates its spirit.

Chimerical cartography was effectively employed in the propagation of ideas by the Nazi geopoliticians. Dr. K. Frenzel, addressing the German Cartographic Society in Berlin, October 22, 1938, declared: "Every map has a suggestive force! Man is an ocular creature. He reacts to that which he sees and can take in at a glance." The private cartographic industry was declared to bear a very heavy responsibility as a mediator between science and the people, and between the policies of the

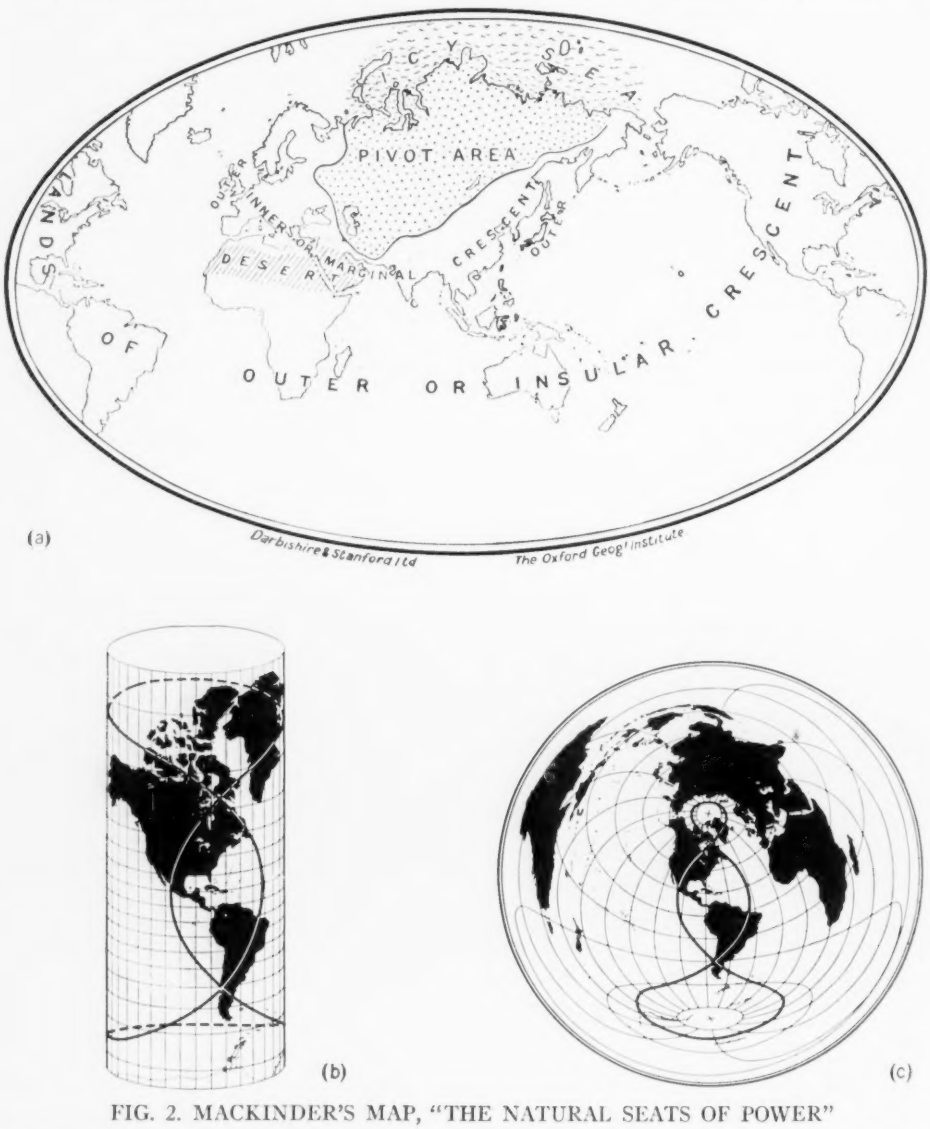


FIG. 2. MACKINDER'S MAP, "THE NATURAL SEATS OF POWER"

(*a*) FACSIMILE OF A MAP BY THE LATE PROFESSOR HALFORD MACKINDER TO ILLUSTRATE A LECTURE ON "THE GEOGRAPHICAL PIVOT OF HISTORY" IN JANUARY 1904. ALTHOUGH ON THE MERCATOR PROJECTION (WITHOUT PARALLELS AND MERIDIANS), IT IS IN THE FORM OF AN ELLIPSE. A PORTION OF THE AMERICAS IS REPEATED AT LEFT AND RIGHT. (*b*) THE ELLIPTICAL LIMITS OF THE SAME MAP SHOWN ON A MERCATOR MAP WRAPPED AROUND A CYLINDER. (*c*) THE LIMITS OF THE SAME MAP SHOWN ON AN AZIMUTHAL MAP. THE LENS-SHAPED AREA IN THE AMERICAS APPEARS TWICE ON MACKINDER'S MAP; THE LOOPS AROUND THE NORTH AND SOUTH POLES ARE WHOLLY MISSING ON THE MACKINDER MAP; THE REMAINDER APPEARS ONLY ONCE ON MACKINDER'S MAP.

government and the people. Every map had to be submitted before publication to all government departments that might have an interest in it. An obligatory organization of several large publishers of school atlases was created in order that unified school atlases would be published for the whole Reich.

Special symbols and devices were developed and standardized by frenetic propagandists, adapted to a minimum of mass education in reading maps, in order to convey ideas of threatening forces, attack and resistance to attack, hostile encirclement, and the like. Posters in railway stations and other public places utilized maps that had a powerful effect upon the uncritical mass of the population.

In Italy cartography was employed by Mussolini to stimulate an urge for territorial expansion. Most striking, perhaps, was the series of maps on a wall in Rome, erected on the Via Imperiale, a new boulevard cut through from the National Monument to the restored Forum. On these maps, which were executed in choice marbles of selected colors, the growth of Rome from a city-state to the empires of Augustus and Trajan was artistically depicted. The purpose was obvious, the method artful. The dominion of Rome once encircled the Mediterranean. Modern Italy's destiny seemed manifest; *Mare Nostrum* was again used with the present tense. No critical appraisal of the lack of pertinence of the extent of Trajan's conquests to the role that can or should be played by Italians in the twentieth-century world was ever tolerated.

The map of Hungary, in a park in Budapest, delineated in a pattern of flowers and foliage which portrayed Hungary's former and current territorial extent, was for years a striking example of cartographic propaganda. Surrounded by stirring words of a famous Magyar poetess spelled out in the foliage and with the national flag nearby always at half-staff in perpetual mourning for territories lost after the first world war this map never allowed the people to accept the imposed territorial changes.

Intelligent use of maps. In a distraught world, whose teeming millions sometimes hesitatingly follow their leaders and would-be leaders as they pick their way among the rubble of shattered cities and ideas, honest and critical thinking about maps is important. Men, women, and even children should all be more critical of the maps they see in daily papers and in periodicals, in books and atlases, and on the screen. They need to be taught to *read* maps (an art in itself) and not merely to *consult* maps—frequently only for location of a single city or point, or regarding a route of travel. Economists, historians, political scientists, and others need to cultivate a keener sense of earth distributions of resources and of peoples and their activities—which necessitates development of ability to read distributional maps.

Cartohypnosis is no more common, however, than delusion and confusion of the mind by subtle uses of words and phrases—but it is perhaps more difficult for the average man to protect himself against the use of mischievous maps. Even the phrases of the most honest men are sometimes inadequate, for, as Whitehead remarked in *Adventure of Ideas*, "The success of language in conveying information is vastly overrated, especially in learned circles."

The map user who desires to guard against becoming the victim of cartohypnosis should keep in mind three things:

(*a*) that it is the actual situation on the earth that is significant;

(*b*) that maps have definite limitations as well as certain unique capabilities; and

(*c*) that map makers are human.

(*a*) It is what one would find on the ground, in all its complexity, and not simply what one finds on a map, that is significant. In looking at a map one may

well ask: What the map shows may be perfectly true, but what is the whole truth? What is on the ground—including peoples and their customs, their ideas, and prejudices? What other types of information are pertinent to the subject?

A small-scale map in a newspaper, designed to indicate territorial transfers and boundary changes, cannot reveal the bilingual populations and the economic and cultural ties between peoples throughout the region. Men could not be so sanguine of solving some of the present perplexing political problems by means of shifting international boundaries, in areas in which boundary changes are ardently advocated (always in other people's territory), if some of the mappable data regarding economic interdependence and cultural transition zones were adequately visualized on maps.

(*b*) Maps have definite limitations as well as unique capabilities. Like an aerial photograph that reveals a pattern, perhaps of archeological origin, almost erased by time and imperceptible on the ground—or like an X-ray photograph—a map may disclose patterns of great significance which are not discernible on surface inspection. Many maps based on statistical data thus reveal pertinent invisible transitions which, if even suspected, would be only vaguely perceived on visiting the area.

A map is unique in its capacity to represent with fidelity literally millions of observed facts, accurately generalized and artistically presented, conveying to the mind a vivid, true picture of the distribution of certain phenomena on the earth's surface that could not be obtained in any other manner. Large-scale topographic maps, for example, if they are highly accurate, belong to this category.

But the limitations of a map should be borne in mind. One of the most important is that a map cannot be more accurate and reliable than the data upon which it is based. A map printed in beautiful colors may be of little value and may mislead the uncritical, especially if it be a work of art. On the other hand, a crudely executed map compilation may be highly accurate and of the greatest importance.

People seldom consider that a map is like a single chapter in an encyclopedic compendium; one map cannot present the results of an inventory of geology, natural vegetation, and water resources. Any map that attempts to show too much is of little use.

Use of the Mercator projection for world maps should be abjured by authors and publishers for all purposes. The world is round. No man ever saw or will ever see a world that has much resemblance to the Mercator world map, and the misconceptions it has engendered have done infinite harm. A map that makes Greenland look larger than all South America, instead of smaller than Argentina, is not suited to portray world relationships. The Mercator is ideal only for navigation, each chart covering a relatively small area. Discrimination should always be exercised in selecting map bases for world maps, the choice depending upon the data or the relationships to be represented.

In this so-called air age, in which men glibly talk of "global relations"—which are misleadingly visualized on *all* world maps, polar and otherwise—one ventures to suggest that the phrase "global geography" should be restricted to those aspects of world relations that can be rationally comprehended, without geometrical acrobatics, only with the aid of globes. I find that transparent plastic hemispheres, some with geographical patterns, and others with geometrical patterns imprinted, which can be moved into any position upon a globe resting only in a cup or ring, provide the best means of comparisons between one part of the globe and another. Map projection distortions and differences of scale are completely eliminated. Having seen a

situation clearly on the globe itself, a map projection may be selected which is adapted to the special requirements of visualizing that particular set of data. There are, to be sure, many types of data which may be grasped when presented on maps even better than on globes. But there are other categories of highly significant relationships, notably the longer ocean-trade routes, air routes and distances, radio and other wave propagations in the field of electronics, and problems relating to the peaceful development of atomic energy for the benefit of all mankind, which require the use of globes and certain types of accessories and actually deserve the appellation "global geography."

(*c*) Maps are made by men, and, as Dr. John K. Wright has remarked (*Geog. Rev.*, October 1942), "Map makers are human." Scientific integrity, painstaking accuracy, and cartographic skill are essential qualifications of the maker of the maps upon which the map user can rely.

Maps that ought to be made and used. Maps are not an end in themselves. If maps can be used as weapons, as Napoleon intimated, they can also serve the needs of peace. Maps can play a unique part as aids in the analysis and solution of complex problems and as tools in planning on community, national, and world scales. One of the most important uses to which maps can be put is to dehypnotize people, to wake them up to the facts and phenomena of the mid-twentieth century, and to educate them to world understanding. Where words utterly fail, maps can sometimes portray, vividly and memorably, some of the freshly and sharply etched, but as yet dimly perceived, lines of interplay between peoples in a world which in many areas is scarcely reminiscent of the conditions upon which our thinking is largely premised. We should bear in mind that until the nineteenth century there were no "world problems."

As a specific example among hundreds that could be suggested: In animated motion-picture maps the areas to which goods produced in Canton can be transported at equal increments of cost can be delineated. As the expanding waves of motion sweep across the oceans and along the railroads of Europe, the Americas, and Africa, they creep almost imperceptibly across China beyond the meager pattern of railroads and motor roads, revealing that villages less than five hundred miles from Canton are much farther away, in terms of transport-cost, than Omaha or Jerusalem. If four-fifths of the people of China cannot trade with each other they cannot trade with the United States and other countries. To see a number of such examples for different continents on animated maps is to grasp the relationship between the lack of modern transport and the "timeworn misery" of low levels of living in large areas of the world.

The world needs maps that visualize economic interdependence of countries and regions; that locate the principal natural resources and their volume of production; that correlate the volume of commerce with decreasing costs of production and transport and that reveal the increases of trade over both short and great distances; that reflect trade balances and international balances of payments; that depict the rapidly expanding patterns of communication in terms of both total and per capita volume; that record the rapidly changing levels of living; that trace migrations of peoples in all parts of the world in recent decades; that disclose the areas in which disease constitutes a threat to health in distant lands—and many other types of maps, including some "maps" on curved surfaces (part-globes) for special purposes.

Resources of governments and of well-supported institutions are needed to underwrite the vast amount of research required in compiling many of the maps that ought to be made. Coordinated programs of map

production are essential if different series of maps are to be readily comparable, and if wall maps, atlas maps, lantern-slide maps, and animated motion-picture maps are to supplement one another. Important technical advances in the science and the art of cartography are desirable and possible. Very significant work will of course be done by individual geographers and cartographers not employed by governments or large institutions. Their contributions will be greater if they associate themselves with economists, historians, demographers, sociologists, political scientists, engineers, and other specialists of many nationalities.

People can seldom be hypnotized against their will. Cartohypnosis can be eliminated as a threat to sane and wholesome development of the world in the interests of its human inhabitants if people look at maps critically and honestly and demand an abundant supply of accurate maps to show them what are the geographical relationships between peoples and their activities.

GENESIS OF WORDS

In restlessness of vague, chaotic thought
In which the self first knows self-consciousness,
Where hope of ecstasy depends on hope
Of words within a wordless wilderness,

For what a satisfaction undefined,
In what despair of hunger unexpressed,
The self, as in a claustrophobic dream,
Cries wordlessly and would resume its quest

For some fulfillment of its vague desire!
What paradox with every hope denied,
That words cannot exist alone for want
Of thought which, waiting for the words, has died!

Here has the groping search of man begun;
Here mark the hope for structure and design—
Hope for the social heritage preserved,
The wise man's book, the poet's singing line.

Here mark the hope for science yet to find
An evidence of self-sufficient cause
And for philosophy to yet record
Some final harmony of cosmic laws.

DOUGLAS E. LAWSON

THE FUTURE OF THE SCIENCE OF ANIMAL BREEDING

By SYDNEY A. ASDELL

Laboratory of Animal Nutrition, Cornell University

MANKIND has one thing in common, an interest in food, how to get more of it of better quality and cheaper. In recent years we have heard much about balanced diets, how to get the best out of the food we have, and of the transport and distribution of food. But we have heard little about the producer's end of it: what science is doing for him and how he looks toward the future. This is especially so in the field of animal breeding and production. To most of us, the domestic animal is like Topsy, it just growed. But there is more to it than that.

Let us take the milch cow as an example. How many of us realize that the average milk yield of a cow in the United States is about 5,200 pounds a year, or considerably less than that of the world's champion goat? What does that mean to the farmer and to us? It means a lot. For the first two years of her life the cow is growing—costing money and returning nothing. It does not cost much more to grow a good cow than it does to grow a poor one, and the cost of maintenance and management is also very little more. Obviously, milk will be cheaper if this overhead is spread over a lot of milk instead of over a little, and the farmer will get a better living and have less work to do because he will have fewer cows to look after. The problem, then, consists in breeding better cows. What is being done to achieve this object? Quite a lot!

A good deal of work is being done to find out the best systems of breeding which, if followed, are likely to result in the best cows. Pedigrees of record-breaking cows have been analyzed to see whether any consistent thread appears in the type of breeding. As a matter of fact, there is no consistent thread. Any system of breeding, except one, produces good results provided the parents are good. The exception is close inbreeding, which is definitely detrimental. Some of the best cows have come from lines in which the degree of inbreeding has been reduced. Similar work with swine, following the work with corn in which many of the best commercial strains are the results of crosses between two inbred lines, has shown that excellent pigs can be bred by crossing inbred lines, but—and this is most important—the inbred lines have to be very good themselves to produce good results when they are crossed. In other words, we have to find and use to the greatest possible extent the best breeding animals. It is easy enough to judge a good cow by what she yields, but it is not so easy to judge the bull because he only expresses his worth in his progeny.

The question narrows itself down, then, to this: how to find the good bulls and how to make the most use of them when they are found. So far, the only way to find the best bulls is to breed a lot of them and determine the milk yield of their daughters. If the daughters are good, the bull is good. By the time we know all this the bull is getting on in years and may not be useful for many years longer. So we are looking for ways to prolong his usefulness and, by better management, to prevent him from becoming vicious in his old age. So far, psychologists have not been called in to help in the latter problem, but I believe they might make a real contribution if they would tackle it. More than two hundred men on American farms are killed each year by bulls.

Increased use of the good bulls when they are discovered is being made by the new

techniques of artificial insemination. By this means, instead of siring 60 calves a year, a bull may sire 500 or more. This gives a much greater sphere of usefulness for the proven sires, and it also speeds up the testing and sorting out of the younger ones. These techniques have passed the experimental stage and are now in practical use. More than 10,000 cows were artificially bred in New York State alone last year, and the number is increasing very rapidly. The introduction of these methods raises a crop of new problems, not the least of which are to keep the bulls fit under this regime and to spot and correct rapid fluctuations in their efficiency.

We are learning how to make more use of the good bulls when we have found them. Can we do the same for the cows? Experiments are in progress to determine whether it is possible by the use of the hormones of the anterior pituitary gland, ovarian stimulants, to cause a cow to shed more eggs than usual and to produce twins and triplets as a rule. This has been done in England under the stress of war necessity, but it is complicated by the fact that a female twin so produced is nearly always a freemartin. Can we transfer the eggs of good cows to less valuable cows so that the latter become internal foster mothers of the calves of the former?

Along somewhat similar lines efforts are being made to induce breeding in sheep, goats, and fur animals outside the normal breeding season and thus to speed up the reproductive processes. Two possibilities suggest themselves: the injection of ovary-stimulating hormones; and the physiological conditioning of animals by modifying the amount of light to which they are exposed so that they live, in effect, in a perpetual spring or fall, whichever their normal breeding season may be.

Besides this work, all directed toward more efficient breeding, the physiologist has not been idle in attempts to increase the milk yield of the individual cow beyond its inherited level. This involves a study of the factors which cause mammary development and secretion, and much progress has been made along these lines. Hormones have been isolated which do what we want, but, so far, they are too expensive for general use and they often do things to other organs which are not so desirable. A cheap synthetic drug, stilbestrol, has proved very interesting as it can be fed instead of injected and it can cause a large increase in milk yield or bring a cow into milk without her calving. But stilbestrol damages the reproductive function rather badly. It does not, however, do this to the goat, which suggests that we may be able to find another synthetic drug that will be as effective in the cow as stilbestrol, but without its undesirable qualities. The hunt is on for such a substance, but we may be up against an inherent peculiarity of the cow.

Another method of approach to the same problem is to find something that will raise the general metabolism of the animal, including that of the mammary gland. Thyroxin is such a substance, and it is the one provided by the animal for its own use. Again, it is expensive, but a cheap substitute has been found and is being used experimentally. One wonders, however, whether the cow will be able to stand up to the strain of an increased rate of living.

THIS last question is very pertinent, and it brings us to another problem that is engaging the attention of the physiologist, namely, the length of productive life. It is unfortunately all too true that the average life of our cows is only a little over SIX years, of which two are used in growing up. Thus, the cow is a milk producer for only four years instead of for ten, which should be her effective life. Again, the overhead is spread over a short period and is, therefore, unduly great. Also, the short life means that most of the heifer calves born have to be drafted

into our herds as replacements, and we cannot do the culling of poor producers that we should be able to do. Some recent work with smaller animals has suggested that by causing them to grow more slowly their length of life may be increased, but we do not yet know whether this method increases the length of productive life, which is the important point. That is the next thing that will have to be determined. On the other hand, there is good reason to believe that slow growth, or perhaps I should say unbalanced growth, injures the reproductive function, and sterility is the cause of huge losses to our farmers. Perhaps we shall have to strike a balance; we may be trying to combine incompatibles.

Sterility in animals is something that has always troubled us and probably always will to some extent. Recent research in this field has suggested that not much can be done to cure it. When a cow is sterile, or nearly so, the damage has been done and it is too late for really effective treatment in a majority of cases. It seems that we shall have to study more closely the effects of different methods of raising calves and of managing them so as to get the best results. Also, there may be a field for the prevention of the diseases that lead to sterility. We may hope that in the future many of these diseases will be wiped out by the development of vaccines. The field is an interesting one, almost untouched, and much work has to be done in it. Naturally, results obtained here may have applications in the human field and vice versa, and so the animal and human physiologists have a common ground and can learn much from each other.

To return once more to the important problems of growth. A rate of growth that is best for prolonging life and productive and reproductive efficiency may not be best for the production of meat. There we want to bring the animals to market condition as rapidly as possible so that the least amount of feed is used up in body maintenance, unprofitable to the producer and consumer alike. Also, we want to make the carcasses such that there is least wastage in slaughter and dressing, and we want the proportions of lean, fat, and bone to be those best suited to the varied market requirements. That means a study of consumer preferences, and in the past it has meant changes in breeding methods to produce the carcass best suited to the market. This the producer has been reluctant to do because to modify an animal by breeding takes many generations of selection. By the time the desired result has been attained, consumer preferences may have again shifted, and the work has gone for naught. Are there short cuts by which we can gain the desired results in a generation? This involves an analysis of the mechanism of growth. When does an animal grow most bone, when most muscle, and when most fat? Can we modify the order of growth in different cuts so that the most profitable cuts grow more in proportion to the less profitable ones? A partial answer to this question has shown that by underfeeding or overfeeding pigs at certain periods of growth the carcass quality can be modified. But the results have been obtained by careful hand feeding where everything is under control. Can we apply such methods so that they can be used practically in mass feeding?

Another approach to the problem is by modifying the activity of the glands regulating growth. This has been done from time immemorial by castration. Can we find other, similar, methods using the experience that has been obtained by the doctor and by the dog breeder? One approach that is being tried is to depress the activity of the thyroid gland, thus making the animal less active, so that he can use more of his food for growth and fattening. By the use of thiouracil for this purpose the fattening of hogs can be brought about more readily, and quite remarkable hams can be grown on small pigs. But the balance is delicate, and

we do not yet know how to adjust the dose and the time of its application to produce these hams without unduly stunting the pigs. How far can we go along these lines?

Another field that is being developed is that of prediction. We already know that, disease aside, a female that comes in heat early and that reproduces readily and frequently at first will usually do better over her lifetime than a poor initial reproducer. How far can we extend ideas of this sort? Can we find early signs of a good milker sufficiently reliable for practical use? The search is on for such telltale signs.

Livestock which flourish in our temperate regions usually deteriorate in tropical regions, whereas the tropics have breeds able to stand the conditions to which they are exposed but with too low productivity to make much contribution to the feeding of tropical peoples. One answer to this question is to modify the two strains by crossbreeding with selection. This is slow and uncertain. What are the constitutional factors necessary to produce a good tropical animal? Temperature control is one of these; disease resistance is another. This avenue is being explored to see what modifications are needed and, eventually, how they may be produced. The answers will probably be partly physiological, partly genetic, and partly a matter of engineering to produce the best conditions for heat loss. Another approach is to compare the feeding stuffs of the two regions, to find out which ones are suitable for different conditions. Should levels of protein, carbohydrate, etc., be modified to produce the best conditions for production?

The physiologist is sometimes asked what he can find to do in the field of animal husbandry. We believe that the problems are many, as we have shown in this article, and that by teaming up with the biochemist, the anatomist, and the embryologist the animal physiologist is making his contribution, and no mean one, to the betterment of animal breeding and the material progress of the world. This applied science is a new one, and, as in all such sciences, much exploratory work has to be done; explanations have to be sought of known facts so that vital processes may be understood and brought under purposeful control.

THE PROBLEM OF PLAN AND PURPOSE IN NATURE*

By GEORGE GAYLORD SIMPSON

The American Museum of Natural History and Columbia University

OF SURPASSING interest to those many minds, which seek after philosophic knowledge and instruction, is the Story of the Earth, Her manifold living creatures, the human generations, and Her ancient rocks." With these words Doughty introduced the second edition of his great book *Travels in Arabia Deserta*. They might serve as a preface to all human learning and especially to that most important of the branches of learning, the study of life. "The human generations," in all their aspects, must be the constant concern of any thoughtful man, whether he aims at "philosophic knowledge" only to enrich his own life or whether he aims at "instruction" to guide him in his functions as a member of society.

In degree, man's social and intellectual complexity is something new under the sun, but man remains a part of nature and is subject still to all of nature's laws. Man is only one of earth's "manifold living creatures," and he cannot understand his own nature or seek wisely to guide his destiny without taking account of the whole pattern of life.

We feel, almost instinctively, that there is a pattern. The diversity of living creatures is neither complete nor random. All living things share many characteristics, and above this basic level we observe groups with every degree of resemblance, from near identity to great dissimilarity. There is, or seems to be, an essential order or plan among the forms of life in spite of their great multiplicity. There seems, moreover, to be purpose in this plan. The resemblances and differences between a fish, a bird, and a man are meaningful. The resemblances adapt them to those conditions and functions that all have in common and the differences to peculiarities in their ways of life not shared with the others. It is a habit of speech and thought to say that fishes have gills in order to breathe water, that birds have wings in order to fly, and that men have brains in order to think.

A telescope, a telephone, or a typewriter is a complex mechanism serving a particular function. Obviously, its manufacturer had a purpose in mind, and the machine was designed and built in order to serve that purpose. An eye, an ear, or a hand is also a complex mechanism serving a particular function. It, too, looks as if it had been made for a purpose. This appearance of purposefulness is pervading in nature, in the general structure of animals and plants, in the mechanisms of their various organs, and in the give and take of their relationships with each other. Accounting for this apparent purposefulness is a basic problem for any system of philosophy or of science.

Attempts to solve this problem are perhaps as old as man. There are few savages so dull or so primitive that they do not have some legend or belief bearing on the problem, even though they do not formulate it clearly or in these terms and even though their solutions are more implicit than explicit. Certainly the problem appears repeatedly, in more or less sophisticated form, among the Greek philosophies, in the Scriptures of all the great religions, and in other ancient attempts to grapple with the nature of the universe and of

* Vanuxem Lecture delivered at Princeton University January 8, 1947.

man. In scientific history, the need for a solution became particularly clear and conscious in the intellectual crisis of the latter part of the eighteenth and early part of the nineteenth centuries.

One possible solution, which had by then become traditional in Western thought and religion, was elaborately presented in the *Bridgewater Treatises*, published in 1833–40. These treatises, of which eight were issued, were richly endowed under the will of the eighth Earl of Bridgewater, who directed that they set forth

> . . . the Power, Wisdom, and Goodness of God as manifested in the Creation; illustrating such work by all reasonable arguments, as for instance the variety and formation of God's creatures . . . the construction of the hand of man, and an infinite variety of other arguments. . . .

They were of unequal merit, but several of them can still be read with pleasure and, indeed, profit. The best of them were, in their day, able works of science, and they strongly influenced scientific thought. (It is noteworthy that although the *Bridgewater Treatises* were, by stipulation, anti-evolutionary, a quotation from one of them stands at the beginning of Darwin's *Origin of Species*.)

The work on the hand, particularly mentioned in Bridgewater's instructions, was written by Sir Charles Bell under the title "The hand, its mechanism and vital endowments as evincing design." Bell did not confine himself to the human hand, but gave a generally excellent account of the comparative anatomy of the vertebrate forefoot and, indeed, of handlike appendages throughout the animal kingdom. He stressed the perfection with which each type of forefoot is adapted to the particular needs and habits of its owner and he pointed out that the intricate mechanism of the human hand follows a seemingly perfect and (as it appeared) obviously purposeful design.

> There is an adaptation, an established and universal relation between the instincts, organization, and instruments of animals on the one hand, and the element in which they are to live, the position which they hold, and their means of obtaining food on the other.

After considering and rejecting the theory of evolution, which was, of course, already well known at that pre-Darwinian time, Bell concluded that

> It must now be apparent that nothing less than the Power, which originally created, is equal to the effecting of those changes on animals, which are to adapt them to their conditions: that their organization is predetermined, and not consequent on the condition of the earth or the surrounding elements.

The fact of adaptation was thoroughly established by such works as Bell's. The tremendous increase in knowledge of nature since 1833 has, on this point, served only to demonstrate that adaptation is even more widespread and may be even more elaborate than Bell knew. To this extent, his arguments are just as cogent now as when he wrote them. But now that we know that evolution is a fact, we can no longer accept his simple solution of the problem of adaptation as reflecting the purpose of a Creator manifested in the separate creation of each species of animal or plant. Whether or not we can explain the evolution of adaptation has no necessary bearing on the truth of evolution. The proofs that have now accumulated, quite aside from attempted explanations of adaptation, are fully sufficient. Competent modern biologists may differ diametrically as to the meaning or mechanism of adaptations and yet all agree that these did, somehow, arise by evolution. If, however, we hope—as of course we do—to go beyond merely observing the course of evolution and to gain some insight into how and why nature has followed this course, then we must still account for the appearance of purposefulness in this history.

In Bell's day, before the proofs of evolution had accumulated sufficiently to carry full conviction in themselves, his argument could only be met by demonstrating the possibility, at least, of an evolutionary explanation of adaptation. Even now we are, quite properly, I believe, reluctant to accept the reality of a phenomenon, be it evolution or extrasensory perception, unless there is some plausible hypothesis as to its mechanism. It is, then, not surprising to find that both Lamarck and Darwin, whose ideas in more or less profoundly modified form have dominated most of the subsequent discussion of evolutionary theory, were at least as much concerned with finding an evolutionary mechanism for adaptation as they were with demonstrating the truth of evolution.

Lamarck's views have been so obscured by the often quite different theories called neo-Lamarckian, by straw men set up by the anti-Lamarckians, and by mere carelessness and forgetfulness, that it is useful to restate his theory. His best expression of it is found in a work published in the year of Darwin's birth, 1809, and grandly titled (in French) *Zoological philosophy, or an exposition of considerations bearing on the natural history of animals, on the diversity of their organization and the faculties that they derive from it, on the physical causes that maintain life in them and that give rise to the movements executed by them, and finally, on causes some of which produce feeling and others intelligence, in the case of those endowed with the latter.*[1]

Lamarck's whole theory embraced five main points, which may be expressed as follows in his own words (translated and rearranged):

1. Nature, in successively producing all the species of animals, commencing with the most imperfect or the most simple and ending her work with the most perfect, has gradually complicated their organization.
2. If the cause that tends constantly to complicate organization were the only one that influenced the forms and organs of animals, the increasing complication would be, in its sequence, very regular throughout. But this is not the case. Nature is forced to submit her works to the influences of the environment, which acts on them, and on all sides the environment causes variations in the products of nature.
3. Whatever the environment may be, it does not directly bring about any modification whatsoever in the form or organization of animals. But great changes in the environment lead to great changes in the needs of animals, and such changes in their needs necessarily lead to changes in their actions. Now, if the new needs become constant or very long continued, the animals acquire new habits, which are as lasting as the needs that gave rise to them.
4. *First law:* In any animal that has not passed the limit of its evolution, the more frequent and continued use of any organ strengthens this organ gradually, develops it, enlarges it, and gives it a power proportionate to the duration of this use, whereas the constant lack of use of such an organ gradually weakens it, deteriorates it, progressively diminishes its capacities, and finally makes it disappear.
5. *Second law:* Everything that nature has caused individuals to gain or lose through the influence of the environment to which their race has long been exposed and, consequently, through the influence of the predominant use of an organ or that of constant lack of use of a part, nature keeps all this by heredity in the new individuals that are born, provided that the acquired modifications are common to the two sexes, or to the progenitors of the new individuals.

[1] The French title is: *Philosophie zoologique ou exposition des considérations relatives a l'histoire naturelle des animaux, a la diversité de leur organisation et des facultés qu'ils en obtiennent; aux causes physiques qui maintiennent en eux la vie et donnent lieu aux mouvements qu'ils exécutent; enfin, a celles qui produisent les unes le sentiment, les autres l'intelligence de ceux qui en sont doués.* The English translation by Hugh Elliot is titled: *Zoological philosophy. An exposition with regard to the natural history of animals, the diversity of their organisation and the faculties which they derive from it; the physical causes which maintain life within them and give rise to their various movements; lastly, those which produce feeling and intelligence in some among them.*

Lamarck's own labeling of the last two postulates as "first law" and "second law" has helped to obscure the fact that these did not constitute the most essential part of his theory but on the contrary were introduced to explain what he considered irregularities or perturbations in the normal course of evolution. In later terminology, Lamarck believed in an orthogenetic progression throughout the whole animal kingdom, tending in a single series from the most simple to the most complex (specified as man), and he believed that special or local adaptations were disturbances in this progression caused by environmental changes and consequent changes in use and disuse of organs, the acquired effects of which are inherited.

Most neo-Lamarckians have rejected or overlooked the broader part of Lamarck's own theory and have supported a general theory of evolution based on the two "laws" that, by Lamarck, were meant only to explain exceptions to the normal course of evolution. Many neo-Lamarckians have also postulated direct effects of the environment on the organism, which were categorically denied by Lamarck. Although I have summarized Lamarck's whole theory here, in an effort to restore historical accuracy, it is mainly his first and second laws that are now of interest to us, also.

Without using that term, Lamarck accepted the purposefulness of adaptation, but he reduced this to evolutionary and to (at least superficially) mechanistic terms by equating the purpose with the needs of individual animals and their efforts to supply these needs. The giraffe has become a stock example, so used by Lamarck himself, and later by Darwin for his different explanation. The "purpose" of a giraffe's long neck is to reach higher leaves and twigs on trees for food. According to Lamarck, the ancestral giraffes stretched their necks in efforts to reach this food—purposeful behavior by the animals. In time, by long repetition, this elongated their necks, the elongation was passed on to their offspring, and this process continued for many generations until it culminated in the long necks of the present giraffes.

This theory of adaptation is ingenious and elegant. Its very simplicity has great appeal, and there is in it something of aesthetic satisfaction, a sort of poetry of evolution. Although it had little influence when first promulgated by Lamarck, later, when evolution had become generally accepted, it was widely discussed and commonly espoused, although it probably never represented a majority view on adaptation. It had a particular attraction for the paleontologists of the latter half of the nineteenth century, and still colors paleontological thought to some extent. Paleontologists found that adaptation was a key to many of their observations and that it was as widespread in the past as in the present. They also found many examples of the gradual, progressive perfection of adaptations that seemed beautifully explicable in Lamarckian terms and they had difficulty in explaining some of these phenomena by the Darwinism of their days.

It is a pity that this thoroughly charming theory is not true, but there can now be no serious doubt as to its falsity. It still has one or two able and eminent supporters—an idea so appealing is not likely to be wholly discarded whatever logic and experience may show—but their battle is clearly lost. The theory indispensably demands that the effects of use and disuse, or other acquired characters, be inherited and that they be inherited in kind and become germinally fixed. That is, Lamarckism requires not merely that modifications acquired during the lifetime of parents should affect their offspring but also that they should affect the same parts of the offspring in the same way as in the parents

and that they should, sooner or later, become a permanent part of heredity in the line of descent, regardless of further or repeated modifying factors. It has been proved, as nearly as a negative can ever be proved, that this does not and cannot occur. Even aside from this fatal point, the evidence against Lamarckism is convincing. There are adaptations, such as those of neuter female insects and many cases of protective coloration or mimicry, for which a Lamarckian explanation is practically inconceivable. The adaptations, like the giraffe's neck, for which Lamarckism does ostensibly provide a mechanism are readily explicable by other processes which, unlike Lamarckism, have been experimentally verified.

I HAVE digressed briefly to show that we cannot now adopt the Lamarckian explanation of apparent purpose in nature, much as we might like to do so. Historically, the greatest vogue of Lamarckism was in the future when Darwin advanced an alternative theory. *The origin of species by means of natural selection; or, the preservation of favored races in the struggle for life*, to give Darwin's greatest work its full Victorian title, was published in 1859, just a half-century after Lamarck's *Philosophie zoologique*.

On different levels, *The Origin of Species*, which is surely one of the most important works ever produced by the mind of man, accomplished three different things. In the first place, it presented the case for evolution, as a general principle in the history of life, and did this so convincingly that it was the most influential single factor in the eventual general acceptance of this principle. Darwin foresaw this result and worked for it, but he knew that the theory of evolution was far from new, and, as its title indicates, this was not, in itself, the primary aim of his book.

The Origin of Species also provided a broad but incomplete explanation of the way in which evolution works. In brief paraphrase, this explanation is as follows: Organisms vary. Some (at least) of these variations are hereditary. Within a given group, some individuals have more descendants than others. The hereditary variations of these individuals are therefore more common in succeeding generations, which tend to evolve in the direction of these variations. It is not clear that Darwin himself definitely separated this theory from the subsidiary theory of natural selection, which is dependent on it. The more general theory is separable, however, and it has been widely accepted as a part of the evolutionary mechanism, even by those opposed to natural selection. Although this particular mechanism certainly is not operative in all cases of evolutionary change, it is just as certainly one of the most widespread mechanisms.

Finally, *The Origin of Species* advanced a theory to account for adaptation in nature, and particularly progressive adaptation. This was the main subject of the book, and Darwin considered it his major contribution, although he recognized that even this was not completely original. The theory is, of course, that of natural selection. The organisms likely to have more descendants are those whose variations are most advantageous as adaptations to their way of life and to their particular environment. Thus evolution is likely to move in the direction of greater or more nearly perfect adaptation, and thus the fact of adaptation, purposeful in aspect but impersonally mechanistic in origin, is explained.

Darwin did not believe that natural selection is a complete explanation of evolution, or even of adaptation. Some students have made much of the fact that he did not exclude the possibility of the inheritance of the effects of use and disuse, in Lamarckian fashion. This point should

not, however, be much emphasized. Darwin was wise enough to see that parts of the evolutionary process were still mysterious, and cautious enough not to rule out completely any supplementary factor that could not then be disproved. He ascribed only an indefinite and very subsidiary role to the effects of use and disuse and he showed that some supposed examples (like that of the giraffe's neck) could better be explained by natural selection.

As Darwin foresaw, his work was followed by the rejection of special creation as a scientific theory. That special creation remains, in some circles, a theological tenet is beside the point here. It is also beside the point, but should perhaps be mentioned in passing, that later scriptural exegesis found no difficulty in accepting evolution, rather than special creation, as God's method of creation and that there need be no conflict between rational evolutionists and rational theologians. After *The Origin of Species,* independent proofs of evolution piled up at an increasing rate, and, equally important, Darwin had demonstrated, more successfully than Lamarck, the possibility of an evolutionary answer to the theological argument of purpose in nature.

The Darwinian theory of adaptation was, however, soon under fire from other convinced evolutionists. One of their main lines of attack was that natural selection, as advanced by Darwin and his successors, is a purely negative process. It destroys but does not create. It eliminates disadvantageous variations but tells us nothing about the origin of advantageous variations and therefore does not, after all, explain how adaptation arises. This merely indicated that natural selection is not the whole story of evolution, which Darwin never claimed it was. It might not have been a very important criticism of Darwinism as Darwin understood it, but some of the neo-Darwinians, toward the turn of the century, did so overemphasize natural selection that this objection did tend to discredit the whole Darwinian school. Aside from this development, the criticism is crucial to our present inquiry because the problem of purpose does not arise from the elimination of the unfit but from the origin of the fit, and it was claimed that the Darwinian explanation of the latter point was discredited.

It was also maintained that characters destined to be adaptive are often of no real use when they first appear, consequently are not then favored by selection, and nevertheless do persist and become useful adaptations. Darwin foresaw this objection to his theory and attempted to answer it, but without complete success. A related objection is the claim that many striking and complex characters of animals have no selective value at any time and so cannot be favored by selection. This, again, would merely mean that selection is not the only influence in evolution, and it has no special bearing on adaptation, as such characters are supposed to be neither adaptive nor inadaptive but merely nonadaptive.

Neither of these latter objections proved to be very serious in the light of increased knowledge of the nature and extent of adaptive advantage, but there was another objection, particularly advanced by paleontologists, that proved to be very serious indeed. After studying numerous lineages among fossils, many paleontologists concluded that evolution tends to proceed in straight lines, that the evolutionary direction of these lines is unswerving even when it has no (evident) adaptive advantage, that it may even run counter to adaptation, and that a trend initially adaptive may continue to such a point that it becomes inadaptive and perhaps fatally disadvantageous.

It was objections of this sort that led many students to believe that attempts

at mechanistic interpretations of evolution, and particularly of adaptation, had failed and were foredoomed to such failure. If characters arise regardless of adaptive significance but nevertheless do, for the most part, come to fill a need, if they do not arise at random but in regular progression, or if they progress steadily in straight lines as if toward a goal—then, it was felt, there must be some guiding force controlling evolution, quite possibly a purposeful force, but this force cannot be found in the environment or in the physical laws of nature. It must either be a force peculiar to life and to this extent non-physical or it must reside on some super-, or at least extranatural, plane. Such a force was repeatedly postulated by students disillusioned with both Darwinism and Lamarckism. Driesch called it "entelechy;" Bergson called it the *élan vital;* Osborn called it "aristogenesis" (among other things); Teilhard calls it "noogenesis." Others call it other names, and some are content to postulate such a force without naming it.

It would certainly be a mistake merely to dismiss these views with a smile or to ridicule them. Their proponents were (and are) profound and able students. Yet in essence what they are all saying is little more than "The cause of evolution is the force that causes evolution." Attempts to define this force usually amount to no more than a description of the way these students suppose evolution to occur, which really leads to no comprehension of how or why it occurs in this way—if, indeed, it does, which is also a decidedly debatable point. As Julian Huxley has remarked, ascribing evolution to an *élan vital* is like "explaining" the movement of a train by an *élan locomotif*. It may be added that "definitions" of these various names for the supposed nonmechanistic causes of evolution generally sound like defining the *élan locomotif* as "that force which causes a train to travel undeviatingly from one station to its goal in another station."

A word must be said, also, on those theories of this general class that go to the extreme of assigning a supernatural cause to evolution, not in the sense of divine establishment of the natural laws of evolution, a belief held by many evolutionists who nevertheless seek to discover these mechanistic laws, but in the sense that the laws themselves are supernatural. Among these views, that of Broom is extreme and yet representative of a trend of thought usually less elaborated and less clearly or frankly expressed. Broom maintains that there is "some spiritual power which has planned and directed evolution" and that below this there are other spiritual agencies, some good and some evil, which in turn direct "partly intelligent" inferior spiritual agencies directly associated with the various animals and plants.

If any of these views are true, we cannot hope to explain "the eternal problem of adaptation," as Osborn called it. They postulate that the purposive element in evolution involves some force that is quite outside the sphere of scientific investigation. We can see what it does but we cannot and need not determine how or why it does this. In fact, these ideas are metaphysical and not scientific, even though this is often denied by their supporters. One cannot but suspect conscious humor when Teilhard (a trained theologian, as it happens, in addition to being one of the most able of paleontologists) states in the midst of one of the most subtly metaphysical discussions of evolution known to me, "In this, note well, nothing metaphysical."

Of course an explanation might be metaphysical and nevertheless be true. It is, however, an obvious lesson from the history of scientific progress that in science one should never accept a metaphysical explanation if a physical explanation is

possible or, indeed, conceivable. In some cases these theories were clearly born of despair and faintness in the search, an emotional state with which we must sympathize but which we should surely seek to avoid in ourselves. Wallace, who independently from Darwin arrived at the principle of natural selection, finally became disillusioned, along with most of his contemporaries, as to the ability of this principle to provide an adequate explanation of evolution. He concluded that "a superior intelligence" must also be involved. Commenting on this after Wallace's death, Osborn wrote of "transformations which become more and more mysterious the more we study them, although we may not join with this master in his appeal to an organizing and directing supernatural principle." Yet Osborn himself, in the course of the twenty-two years that then remained of his long and fruitful life, came to expound evolution as the result of an organizing and directing principle which he refused to call supernatural but for which he found no naturalistic basis or explanation.

Rise of modern knowledge of heredity in the early years of this century brought with it an attempt to dispose of this troublesome, eternal problem by abolishing it, an astonishing solution as seen in retrospect and yet one that held sway among the most advanced biological theorists until quite recently. It was found that new characters and new types of plants and animals can arise quite suddenly by some change within the hereditary substance in the germ cells. As far as is known, these changes, mutations of various sorts, arise quite at random. The geneticists, fervently exploring on the frontier of this newest offspring among the biological sciences, proposed on this basis to do away with all the musty problems of purpose or adaptation by simply denying their existence. New types of plants and animals arise at random, and that is all there is to it. No adaptation and, of course, no purpose.

The geneticists quickly took a great step forward in the attack on one very essential phase of the problem of adaptation, the question of the nature and origin of hereditary variations. Lamarck faced this question more squarely than almost any later student of the nineteenth century, and he proposed an answer, but his answer was wrong, as the geneticists, among others, have clearly shown. On the whole, Darwin avoided the question. It is doubtful whether even a partial answer was practicable in his day, and to this extent Darwin's study of adaptation was premature, but his evasion or glossing over of the question was a serious defect in his theory. It led to some confusion among his followers and to a reaction against this concept during the next half-century. The vitalists met the question in a still less satisfactory way, by relegating it to the realm of the unknowable or by self-deceiving naming fallacies.

The geneticists have observed and isolated nascent variations (mutations of various sorts), and they have found out the more essential facts about how these are passed on, combined or separated, and expressed in later generations. Beyond these processes there are other, deeper, mysteries into which the geneticists have barely penetrated, if at all—all steps toward greater knowledge bring us newly to the threshold of more profound mystery. But modern geneticists have supplied what seems to have been the last lacking basic information necessary for an explanation of evolution essentially complete on, at least, the descriptive level. Their own attempts to apply this information to evolutionary theory were not, at first, fruitful, although we shall see that they later became so. In earlier years of the present century most geneticists worked in (mainly self-imposed) isolation from the paleontologists, systematists, comparative anatomists, and others

who had accumulated a vast body of data on the course and on the results of evolution. As is so likely to happen when great new discoveries are made, some of these geneticists were overenthusiastically sure that their discovery quite superseded all that had gone before and was the sole and whole key to evolution. In no other way can one understand their proposed solution of the problem of adaptation by saying that there is no such thing as adaptation.

Adaptation does exist and so does purpose in nature, if we define "purpose" as the opposite of randomness, as a causal and not a merely accidental relationship between structure and function, without necessarily invoking a conscious purposeful agency. Denial of this does violence to the most elementary principles of rational thought. Look again, with Sir Charles Bell, at your own hand, manipulate the fingers, think of the intricacy of the combination of bones, muscles, tendons, blood vessels, and nerves in this mechanism and of the complexity and delicacy of its conscious control. How can one estimate the improbability that such a structure arose by sheer accident, or by any continued series of accidents short of infinity? The example is simple but it is convincing in its very simplicity. Consider, then, all the other structures and organs of man and of all the other animals and plants and the way in which these function and serve the many special needs of each type of organism. Consider, too, the interrelationship of these organisms, the dependence of animals on plant photosynthesis, the social groups of ants, the fertilization of flowers by insects, and the myriad other adaptive ecological relationships. Whatever improbability you assigned to the random origin of your hand, this might be multiplied by a billion billion to express the improbability that nature as a whole is the result of any sequence of accidental and random events. It is necessary to agree with Julian Huxley that "to produce such adapted types by chance recombination . . . would require a total assemblage of organisms that would more than fill the universe, and overrun astronomical time."

Although some geneticists really were, for a time, so naive as simply to deny the reality of the problem of adaptation, it would be unfair to imply that this opinion became universal or endured long among them. Acceptance of random mutation as the mechanism of evolutionary change and of the fact that adaptation—or something that looks singularly like it—is well-nigh universal in nature led to emphasis of another evolutionary principle, that of preadaptation. The essence of this principle is that structural or physiological peculiarities, although arising at random, may prove to be suitable or useful in the environment of the organism or in some other environment available to it. For instance, animals living in fresh water may by mutation become tolerant of salty water and then spread into brackish streams or into the sea because they now can; or animals living on a forest floor may undergo mutations enabling them to climb and may then take to the trees for refuge. This is a sort of reversal of the old idea of apparently or really purposeful adaptation in, for instance, the Lamarckian sense. The "adaptation" (it should probably be in quotes when used in this way) comes first and its use follows. According to an extreme version of this theory, all strongly distinctive types of animals originated as sports, or "hopeful monsters," as Goldschmidt calls them, that happened to find a practicable way of life adapted to their peculiarities, rather than originating by any process that adapted them to peculiarities of the environment.

This is an interesting and important theory. There is little doubt that preadaptation does really occur, although rarely if ever in the extreme form supported by Goldschmidt. We shall see that preadaptation, with some expansion and modification of its

significance, must be accepted as one of the general factors in the new synthetic theory of evolution. Earlier opinions that random preadaptation is an adequate explanation of adaptation were, however, quite unjustified. Objections already expressed to accepting any explanation of adaptation as a result solely of random, accidental, or unsystematic processes apply with equal force to preadaptation in this relatively crude form and without any additional postulate involving a systematic guiding influence and providing for pseudo- (if not really) purposeful progressive increase in adaptation.

AT THIS stage in the inquiry, the situation seems almost hopelessly confused. This was the position only a few years ago, with many special theories ardently supported, although to a dispassionate observer none would have seemed adequate and some would have seemed absurd. Some students, a very small minority, denied the existence of adaptation. A host of other students had, however, demonstrated really beyond any doubt that adaptation does exist and that a great majority of the characters of animals and plants, although not all their characters, must be considered as definitely related to, and requisite for, their particular ways of life. For some this was merely an inexplicable fact; these students were few, because scientists rarely are psychologically capable of accepting a phenomenon as a fact and also accepting it as inexplicable. It is well for the progress of science that this is so, but it has led to many premature and to some bizarre theories.

Die-hard neo-Lamarckians still maintained that adaptation is explicable as the result of the effects of the environment on the organism and of the organism's own efforts to cope with the environment. Die-hard neo-Darwinians still maintained that the removal of misfits by natural selection is the whole story. The latest comers, the geneticists, were in many if not in most cases convinced that the only adaptation is preadaptation. Besides these three main mechanistic schools, there were many who supported a wide variety of covertly or overtly nonmechanistic views. In part they were reacting in a natural way against the excesses and deficiencies of each of the mechanistic schools. Rationally unable to accept one or the other of these, they were unwilling to leave adaptation unexplained and sought refuge in nonscientific or metascientific theories. In other cases they were influenced by, or returning to, the preevolutionary and long antievolutionary concept of adaptation as literally purposeful but, as a rule, they dodged the question of who or what formulated the purpose.

The story of the elephant and the blind men, who argued bitterly over the nature of the beast only part of which was known to each of them, is old and trite. It has become trite because it is so often apt, and it is very apt here. Each of the diverse schools of thought about adaptation knew or emphasized only part of the pattern. Each tried to complete this pattern as a reconstruction from a part only and each bitterly rejected the reconstructions of the other schools, based on other parts. The neo-Lamarckians knew and overemphasized the fact that adaptation is pervasive in nature and essentially purposeful in aspect, as if the environment had forced and the organism had sought adaptation. The neo-Darwinians knew and overemphasized the fact that the more or less adaptive status of variations is influential in determining the parentage of a following generation. The geneticists knew and overemphasized the fact that new hereditary variants arise abruptly and, as far as we know and as far as adaptive status is concerned, at random. The various non- and antimechanists knew and overemphasized the fact that adaptation is usually an essentially directional, progressive, sustained, and nonrandom process.

Once the search has been summarized in this way and its outcome expressed in these words, it becomes obvious how to seek further for an answer to the problem of plan and purpose in nature, or of adaptation, to which the problem is reduced by elimination of some of its metaphysical overtones. What was necessary was synthesis, bringing together the facts and theories of all the schools, accepting those mutually consistent and reciprocally reinforcing, rejecting the inconsistent, inadequately supported, and unnecessary, and building anew on this basis. Such a development, obvious as it now seems, would have been premature until rather recently because essential elements, especially some of those lately supplied by the geneticists, were still lacking, and the synthesis would still have been so incomplete that it would have satisfied none of the participants. When time was ripe for it, it was still impeded by the extreme specialization that had grown up in biology as in other sciences. Students in the various narrow specialties tended to be ignorant of any field but their own and were sometimes downright hostile toward any other.

The breaking down of these barriers, a necessary preliminary not only for further progress in the study of evolutionary adaptation but also for true comprehension and integration of all fields of biological research, is a recent development. A precise beginning can hardly be designated, but this movement was presaged some ten or fifteen years ago by individual efforts, mostly English and American, to renew, broaden, and modernize the attack on evolutionary problems. More recently it has been put on a wider basis and has become a conscious collaboration between many specialists in different branches of biology. In this country, this has been promoted by the National Research Council Committee on Common Problems of Genetics, Paleontology, and Systematics. The work of the committee is broadened and put on a permanent basis by the recently (1946) established Society for the Study of Evolution.[2] A high point in this movement toward cooperation and synthesis and a powerful stimulus for future work were achieved by the recent Conference on Genetics, Paleontology, and Evolution held at Princeton University.

Those of us engaged in this movement find the results so exciting that we may unintentionally exaggerate their extent and importance. It is quite clear, however, that the study of evolution, viewed with apathy and even considered wholly outmoded by many biologists a few years ago, now attracts more attention than at any other time in the history of science. The future alone can judge, but it also seems probable that more sound progress has recently been made toward understanding the processes of evolution than in all the previous centuries of study put together.

The result, to this point, is essentially a new theory of evolution, or a new body of theory, although its basis is a synthesis of previous theories. It has been called neo-Darwinian because it includes the Darwinian factor of natural selection and excludes the Lamarckian factor of inheritance of acquired characters, but this is a misnomer and is likely to mislead seriously. It involves, and to a considerable extent it grew out of, rehabilitation and restatement of the principle of natural selection in genetical and statistical terms, but its understanding of natural selection is quite different from that of Darwin and still more different from that of the neo-Darwinians. It also embraces a great deal more than natural selection. It is more complex than any one of the previous

[2] With the aid of a grant from the American Philosophical Society, the Society for the Study of Evolution has also established an international journal, *Evolution*, Volume One of which is being published in 1947.

theories; their factitious simplicity was a weakness and a cause of their failure. It can hardly be labeled with the name of any one man, and an outstanding characteristic is precisely that it is the product of many minds and has drawn data and principles from many fields. It may perhaps most appropriately be called simply the synthetic theory.

It would manifestly be impossible to present an adequate statement of the synthetic theory in a simple or brief way, and this would not be appropriate in this broad review of a single, even though of a very basic, problem of evolution. Various aspects of the theory have recently been treated at book length by several students from different points of view, among them Dobzhansky, from the point of view of a geneticist, Mayr, from the point of view of a systematist, Julian Huxley, from the point of view of a neobiologist or naturalist, Stebbins, from the point of view of a botanist, and Simpson, from the point of view of a paleontologist. Equally or more important studies have also been made by Muller, Sewall Wright, Haldane, and R. A. Fisher, and fairness would demand, but unfortunately space prohibits, the mention of dozens of other names.

To elucidate, as far as can be done in a brief summary, the synthetic theory's solution of the problem of adaptation, it is well first to restate the problem as it appeared to the opposing teams which, by combining their efforts, produced the synthetic theory. The geneticists had, as they thought and as we paleontologists, systematists, and other nongenetical naturalists have come to accept, identified the materials for evolution. These materials are discrete changes in the hereditary mechanism which is, in the main, an elaborate organization of chemical substances into units called genes, which are assembled into united groups called chromosomes, which in turn occur in sets one or more of which are present in every cell that is to grow into an organism. These chromosome sets as a whole, with all their constituent parts, determine what the organism will become as it grows. The geneticists found, moreover, that the changes in this mechanism, those changes that are the materials for any evolutionary change, are not predominantly adaptive. On the contrary, they have no particular orientation toward adaptation but are random in this respect. Still worse, this means that the great majority of them are definitely opposed to adaptation, because in any organism the number of possible changes toward increased adaptation or toward new adaptations is very much less than the number of possible changes away from adaptation. Moreover, what an organism is like is determined by its whole set of chromosomes. Adaptation rarely requires a single new mutation, but an entire set coordinated in a new way. That genetic mutations should produce a new sort of organism adapted to an available environment is improbable in so high a degree as to seem almost impossible. Even granting that natural selection would, in Darwinian fashion, weed out the grossly unfit genetic combinations, the fit simply would not arise, or would arise so extremely infrequently as to be altogether exceptional in nature.

On the other hand, the set of facts known to the rest of us, the nongeneticists, demonstrates beyond any possible doubt that these almost impossibly improbable genetic combinations are in fact so common in nature as to be nearly universal. The only possible way to reconcile the facts of genetics with the facts of adaptation is to find some force or process in nature that is capable of generating a high degree of improbability, as R. A. Fisher has put it or, in other words, of assuring that an outcome that is genetically extremely improbable will nevertheless become usual.

This force has been identified beyond reasonable doubt, and it turns out to be an old friend (or enemy), natural selection, but

natural selection on a new basis and in a new role, a process subtly but fundamentally different from the natural selection of Darwin or of the neo-Darwinians. It is not merely the negative process of elimination of the unfit, by assuring that they will have fewer offspring than the fit; it is the positive and creative process that was left out of the picture by the Darwinians and that was sought in vain by the Lamarckians, the vitalists, and others.

How natural selection works as a creative process can perhaps best be explained by a very much oversimplified analogy. Suppose that from a pool of all the letters of the alphabet in large, equal abundance you tried to draw simultaneously the letters *c*, *a*, and *t*, in order to achieve a purposeful combination of these into the word "cat." Drawing out three letters at a time and then discarding them if they did not form this useful combination, you obviously would have very little chance of achieving your purpose. You might spend days, weeks, or even years at your task before you finally succeeded. The possible number of combinations of three letters is very large and only one of these is suitable for your purpose. Indeed, you might well never succeed, because you might have drawn all the *c*'s, *a*'s, or *t*'s in wrong combinations and have discarded them before you succeeded in drawing all three together. But now suppose that every time you draw a *c*, an *a*, or a *t* in a wrong combination, you are allowed to put these desirable letters back in the pool and to discard the undesirable letters. Now you are sure of obtaining your result, and your chances of obtaining it quickly are much improved. In time there will be only *c*'s, *a*'s, and *t*'s in the pool, but you probably will have succeeded long before that. Now suppose that in addition to returning *c*'s, *a*'s, and *t*'s to the pool and discarding all other letters, you are allowed to clip together any two of the desirable letters when you happen to draw them at the same time. You will shortly have in the pool a large number of clipped *ca*, *ct*, and *at* combinations plus an also large number of the *t*'s, *a*'s, and *c*'s needed to complete one of these if it is drawn again. Your chances of quickly obtaining the desired result are improved still more, and by these processes you have "generated a high degree of improbability"—you have made it probable that you will quickly achieve the combination *cat*, which was so improbable at the outset. Moreover, you have created something. You did not create the letters *c*, *a*, and *t*, but you have created the word "cat," which did not exist when you started.

Creative natural selection works in a similar but vastly more complicated way. The number of possible combinations and arrangements of genes and chromosomes in a group of organisms, even of relatively simple organisms, is enormous, so enormous that most of them will never occur because the number of organisms in the group is much less than the number of possible genetic systems. Clearly the critics of natural selection, in the old sense, were quite right in concluding that merely eliminating inadaptive combinations would rarely, if ever, insure the appearance of adaptive combinations. A mutation favorable in itself or in some particular combination would quickly be swamped among the inadaptive mutations and would usually be eliminated because it occurred in unfavorable associations. But it has been demonstrated both theoretically and experimentally that selection acts in a positive way tending to increase the percentage of favorable genes in a population, a process analogous to your increasing the proportion of *c*'s, *a*'s, and *t*'s in the alphabet pool. It thus greatly increases the chances not only of favorable single genes but also favorable hereditary combinations. Moreover, selection also acts on combinations and arrangements of genes. Just as you clipped together the "adaptive" sets of letters *ca*, *ct*, and *at*, selection prevents adaptive combinations of

genes from being dispersed again and increases their frequency in the population, thus promoting the probable development of still more complex, adaptively still more favorable, combinations. It has also been demonstrated, to meet another old criticism of natural selection, that under certain conditions (conditions that do obtain in natural populations that are well adapted) an extremely small selective advantage suffices to insure that favorable genes and genetic systems will be preserved and will increase in frequency.

Another major criticism of natural selection, in the old, Darwinian, sense, is also being well answered by the studies of, particularly, paleontologists and geneticists. This criticism, you will recall, was that evolution exhibits trends that proceed without deviation ("orthogenetically," in paleontological cant) and that may be inadaptive or may proceed well beyond the point of greatest adaptive advantage. In some cases these trends simply did not really exist or were not really the single, straight-line affairs that they had been thought. W. K. Gregory and others have well shown that many of these straight lines are more a product of scientists' minds than of nature. In other cases, the interpretation of some features of these trends as inadaptive seems to have been due to an inadequate, sometimes a naive, conception of adaptation and of heredity. One factor here is that the hereditary mechanism makes it quite possible for an unfavorable trend to become associated with a favorable trend and to be carried on with it as long as the balance continues favorable. There are some remarkable complexities involved in this subject, but it seems reasonably clear that long-continued trends in general are kept going by natural selection.

The action of selection, understood in this newer way, results in the appearance and spread of genetic systems and therefore of sorts of organisms that would never have existed under the uncontrolled influence of mutation and random recombination of the elements of heredity. In this sense, although it does not create the raw materials, the mutations, natural selection is definitely creative. It creates the most important product of all, the integrated organism. Builders do not make bricks, but they create houses, and the bricks are not adapted into a use until they are assembled into houses. Natural selection is a builder that uses mutations as bricks, and its constructions are adaptive. The figure of speech would be still more apt if builders had to find their bricks in piles of rubble in which most of the pieces were inappropriate for their purpose.

The whole process and its results are much more intricate than is apparent from this simplified explanation, and natural selection is by no means the only process involved in evolution or even in adaptation. Creative natural selection is the directive, pseudopurposive factor back of adaptation, but it is not always the decisive factor in evolution and it never acts alone. Other factors explain, for instance, why not all characters of organisms are adaptive, why not all newly evolved sorts of organisms are adaptively superior to their ancestors, and why organisms adapted to essentially the same activities and environmental conditions may nevertheless be strikingly different in many respects.

Among the other processes of evolution, one particularly pertinent to the present theme requires further, brief, comment, and that is preadaptation. Preadaptation commonly occurs in evolution and is sometimes of crucial importance, but it is not as universal as was believed by its earlier proponents, and its role and the factors underlying it are rather differently understood in the synthetic explanation of evolution. It now appears that preadaptation may arise in two or three quite distinct ways, none of which is wholly random or

spontaneous, as all preadaptation was once thought to be. Occasionally one mutation, using the word in a broad sense, may produce a relatively great difference from the parents in the organism affected by it, and this difference may be adaptively favorable for available new habits or environments. This is, in a sense, preadaptation, but in the same sense any useful mutation is preadaptive, and this application of the term is not enlightening. Once such a mutation has arisen, it is simply another bit of material for natural selection and its status is the same as any other mutation except for its larger size, in terms of structural change. Large or small single-step adaptations, as Julian Huxley calls them, seem, however, to be of relatively little importance in evolution as a whole. They explain occasional evolutionary events, but the usual course of adaptation is slower and more cumulative.

One type of preadaptation, speaking more strictly, may occur when selection becomes relatively ineffective and the frequencies of the various genes in a population tend to drift, aimlessly as it were, without definite control by selection. This has been called the Sewall Wright effect, after one of the eminent cofounders of the synthetic theory, who has demonstrated that this effect is most likely in small, isolated groups of organisms. In most cases the changes brought about in this way are inadaptive, and they are usually a prelude to extinction, but occasionally they are preadaptive. Then they may facilitate a relatively abrupt and radical change in habits and environment, a change of the sort that I have called quantum evolution. It may be that many of the major and broader new types of organic structure arose in this way. This is probably the nearest thing to purely random preadaptation in the old sense, but even this cannot be considered wholly random. The structure from which such developments depart owed its integration to (nonrandom) selection, and the new grade of structure must, if it survives, in turn be integrated by selection which will act with particular force on such groups.

Another, and probably a more common if not more basically important, sort of preadaptation arises when, for instance, a selectively controlled adaptive structure develops to the point where its use in a new way becomes possible. Then it is preadaptive with respect to its new use, and selection will then direct evolution in this new direction if the new use is advantageous. This sort of preadaptation is not random at any point. It is always directional and directed, but the direction changes. Such occurrences demonstrate how the direction of evolution can change under the influence of selection even though the environment remains essentially constant.

Adaptation by natural selection as a creative process and preadaptation in the special senses just explained are the answers of the synthetic theory of evolution to the problem of plan and purpose in nature. Of course much work remains to be done, many details to be filled in, and many parts of the process to be more clearly understood, but it seems to me and to many others that here, at last, is the basis for a complete and sound solution of this old and troublesome problem.

Adaptation is real, and it is achieved by a progressive and directed process. This process is natural, and it is wholly mechanistic in its operation. This natural process achieves the aspect of purpose, without the intervention of a purposer, and it has produced a vast plan, without the concurrent action of a planner. It may be that the initiation of the process and the physical laws under which it functions had a Purposer and that this mechanistic way of achieving a plan is the instrument of a Planner—of this still deeper problem the scientist, as scientist, cannot speak.

THE SCOPE OF SCIENCE

By RALPH W. GERARD

Department of Physiology, The University of Chicago

SCIENCE, like a flooding sea, has washed over many successive shore lines toward man's intellectual horizon. Today, more than three centuries after Newton, we can still say, with him, that an ocean of truth lies before us—but not "all undiscovered." How extensive the unknown remains, how quickly or slowly man will chart it, however, is not our present concern. This is rather with the unknowable than the unknown or, more precisely, with the necessary limits of natural science and its methodology. For the exuberant optimism of the last century, which saw progress to Utopia as man's destiny and science as the means to it, has, in disappointment and impatience, given way to a mounting distrust of the rational and natural and to a resurgence of faith in the supernatural and divine. As when any political party retains power long enough for its imperfect performance to become apparent, or even for fortuitous distress to fall upon the people, the citizens "turn the rascals out," so mankind is now inclined to turn upon man's reason and its representative, Science.

Some thinkers of today insist on subordinating scientific knowledge to a higher "wisdom," as does Maritain, or even denounce science and its attitudes as the "central corruption" of modern times, as does Adler. Others set sharp limits to the methods of natural science: they are not applicable to the study of mind, says Blanshard; or society, say Knight and Novikoff; or history, say Becker and Barzun; or values—moral, aesthetic, or rational —say most humanists. With these positions I propose to take issue, and as sharply as possible, to aid clarification. I am not concerned to exalt my own field of endeavor for parochial pride or privilege; the most restricted acreage one could allot to science would still keep scientists busy cultivating it for many generations. Perhaps this essay exemplifies only "that fierce passion for monism which burns in many scientific breasts" (Woodger). Or perhaps it is an honest effort to avoid "the error of misplaced concreteness" which results from the greatest sin of the intellect, "the intolerant use of abstraction" (Whitehead).

THE NATURE OF SCIENCE

First, what is science? Not merely the work of men in the ranked laboratories on our campuses, nor of men everywhere in laboratories or on campuses. It is wider and deeper than that. It is a state of mind or philosophy, and a procedure, and the results of these. It aims "to translate experience into general laws of predictive value." "Value," in this phrase of Malinowski, implies the more abstract values of science: predominantly truth; with utility (a form of good, if not the whole of it) in increasing focus; and beauty, not absent, but background. It attacks, and solves, problems by imagination applied to sensory evidence and "curbed and ruddered" by reason. Science is the outcome of the Hellenic break with the more primitive "mythopoeic" mind; of seeing nature as impersonal and implacable and so regular, rather than personified as "I and thou," as Frankfort puts it, and so capricious.

Science is a creation of man and is a work of art. It requires the same talents of mind and hand and results in the same generation of new form or structure as do other artistic

productions. To be sure, the particular ingredients needed for achievement differ in proportion and detail, and an individual may be more fit for one type of endeavor than for another. Also, because of its highly cumulative and cooperative character, artisans or technicians play a greater part in science than in some forms of art. Perhaps science is closest to architecture, which combines use with beauty and requires collective effort at many levels of creativity. But it would be an error to consider technology as unimportant even in the most individual arts; where would they be without chisels or strings or paper, without printing and radio, without, for that matter, the tool of language itself? As Shotwell has well put it, we "need a *Sartor Resartus* in the history of literature to show how naked and helplessly limited is thought except when provided with mechanism." (This, to anticipate, is equally true for supplying fresh ingredients to think about as for expressing the resultant thoughts. Technology enhances the sense organs of man and enriches his experience, no less than it gives power to his expression.) Shotwell further distinguishes two kinds of creations of value, one possessing "monumental immortality," the other "useful immortality." The former are enduring objects, statues or buildings or dams; the latter, enduring ideas. The ideas are more important in science and some arts; the objects, in technology and other arts. Perhaps one reason the line between the sciences and the humanities is so blurred is that humanistic thought has itself been strongly influenced by the attitudes of science; or, if you prefer, the Hellenic mentality has created both in its image.

For science and scientists, like art and its devotees, are products of man's culture, as they are also its molders. They are differentiated mechanisms and units of the human epiorganism. They have come into existence by action of the same forces of evolution that earlier produced man and his thinking brain. In detail, the science of an epoch can be traced back to the social pressures of its time, as Crowther and Hogben have emphasized, and forward into the social pressures of the following period. The scientist may be looked upon as the sense organ, or receptor, of the social organism which, like the receptor of the biological organism, develops as an adaptive response to environmental influences and, developed, accelerates further adaptation in the whole organism—an autocatalyst of evolution. And it were well to note, at this point, that science transcends the individual man or mind as every organized system does its component units.

I certainly cannot prove that the scientific view of the universe is the best or the final metaphysic which man will reach, or that truth is a crowning value. But I do maintain that this approach, of those man has tried, has led to great and progressive change in human affairs—mental as well as physical—and that, until an approach more satisfactory by empiric standards arises or the limitations of the present one are clearly demonstrated, man had better "hold fast that which is good."

DIVISION OF SCIENCE

Only such broad descriptive sweeps deal with science as a collective entity; for the particular sciences differ from one another enormously in detail. The units of concern, the concepts which prove useful, the methods for study are, in practice at least, fairly unique to each area. A good yardstick for yards is likely to be a poor one for health. The method of breeding in biology is as different from that of timing in physics as is the mental test or sociological questionnaire from breeding. Even if all could be accurately measured, the "pointer readings" would still be vastly different for volts,

vitality, and values. Further, at present, the degree of quantification, of logical compulsion of available evidence, and of predictive assurance varies greatly from segment to segment. Yet within the proper scope of science these particularities are secondary and might, with sufficient knowledge, reduce to continuities. Which brings us sharply to our primary problem.

SCOPE OF SCIENCE

Historical. It is all too obvious that the Newton of mind or the Darwin of society or the Lavoisier of values has not arrived. The question is: Is he possible? I maintain that he is; and that those who deny such a possibility are no more than modern Bishop Wilberforces. Look at the past; what pessimists have men been! When regularity and predictability were established for the simpler material phenomena on earth, when geo-metry was developed and the action of levers understood, celestial events were still considered beyond man's understanding and daring. For a millennium, the angels still moved the heavenly bodies on their ways. Not until Newton demonstrated the universality of application of his equation for gravity, indeed not until Bunsen, a scant century ago, showed by spectroscopic analysis of sunlight that the same elements compose the sun as the earth was this tidemark finally flooded over. (Kirchner, considering that water might occur on other planets, earnestly raised the question whether a baptism with water from Venus would be effective.) Today, no vestige of thought leaves our planet unique in the universe—even as a habitation for life, none question the possibility of other similar satellites scattered about the galaxies; yet the best minds of the past, as good as those of today, were emphatically convinced that such matters were forever beyond the grasp of man's understanding and must be settled by faith.

Or, consider the problem of the living. Almost to our day, if not actually into it, the animate was shrouded with the mysterious and the impenetrable. Vital spirits, presiding entelechies, a whole corps of benevolent gremlins made the parts of the body go through their chores and the whole execute the beautiful acts of living. Why, even in my own student days, secretion was an expression of the vital activity of glands; now it is analyzed in terms of oxidative energy, ionic interchange, differential permeability to polar molecules, and the like. A little over a century ago, the substances of the living were forever beyond the province of the chemist, a vital force alone could make "organic" compounds. Then Wöhler made one in the test tube, and the chemical hordes, given hope and courage, poured after with hundreds more. Yet organic compounds *are* different in many important respects from inorganic ones, and it is only in the immediate present that the underlying laws of molecular organization are reducing these differences from the qualitative to the quantitative. (This is an especially convincing instance to support the view that, at all levels, the qualitative jump will in time become a quantitative difference and that "prediction upwards" is not impossible but only enormously difficult.) Again, the gross structure of organisms was well known to Galen and was described in modern form by Vesalius four centuries ago; and even its microscopic structure has been limned out for three hundred years. Yet our knowledge of function was largely nonexistent before Harvey and is still expanding at an accelerating rate—along with the unraveling of submicroscopic organization.

When it became clear that vital phenomena were not outside the grasp of science, new boundaries were set by the pessimists. The properties of the individual brute organism, yes; but its origin, development, inheritance, never! Plants and ani-

mals, yes; but man and mind, never! In 1909 (or actually 1930, in the English translation) Radl could say in his critical *History of Biological Theories:*

> No observation, experiment, or intellectual speculation can alter the fact that each organism is an autonomous individual; it contains within itself the laws of its existence; and, in spite of the influence of its surroundings, it develops along its own lines. As to the nature of the directive force we know nothing.

Also he says, "In recent years the fantastic idea that we come nearer and nearer to reality, the more we magnify the structures we are observing, has lost ground." With electron microscopy, X-ray diffraction, and still newer methods just now beginning to reveal the molecular machinery of protoplasm! Yet he makes this vivid comment on the reception given Darwin's views even by scientists best qualified to follow them:

> But few dared ask how the creation of these various forms of life, or of the whole living Universe, came about. They felt that human understanding was powerless to attempt an explanation—too much was known of Nature's impenetrable mystery and of her infinite power. Then came Darwin . . . he taught that life has continued since the beginning in one unbroken stream, and that the origin of new forms and extinction of old ones can be explained without invoking any factors which surpass human understanding. Many of the paleontologists of the time regarded the theory almost as a personal insult!

That battle to unbind inquiry is won, except for some mop-up action in the intellectual back country. But the barricades, or, returning to the earlier figure, the dikes, are still actively manned to defend the territory of the human and the mental from the encroachments of science. These we shall examine later; now the more general problems of scope require attention.

GENERAL LIMITATIONS

Science has limited aims, it is directed toward understanding rather than feeling. Even if in time it becomes possible to explain fully the factors that enable a particular masterpiece to stir the awareness of beauty in some individuals and not in others, this will still be separate from creating or appreciating masterpieces. True, such understanding should make easier and more certain the construction of new forms which will arouse desired feelings; but the creating and the feeling remain separate from it. So also for objects of utility rather than beauty; understanding makes possible their initial or efficient production, it is not necessarily concerned with their manufacture or use. Science has learned much of how caffeine acts on the brain, with little influence on coffee-drinkers; it has learned much of how pneumococci act in the body, with tremendous influence on the expert treatment of pneumonia.

Reason and emotion are more than traditional or vulgar terms of crude description. They can be characterized by more rigorous psychological and physiological criteria and are fairly sharply separated in brain structure and function. Emotional behavior, and probably consciousness, depend on the activity of phylogenetically ancient brain parts, which are similar through much of the vertebrate subphylum. The structures, the behavior, and presumably the basic awareness involved in fear, rage, pain, and the like are comparable for most individuals of many species. The emotions are aroused and discharge themselves with the same sort of predictability and inevitableness that characterize simple reflexes. They are mainly of significance for self-survival and are private. The feelings of an individual cast a small glow in his immediate vicinity, since others recognize that he is angry or elated; but even they cannot enter into his current experience, though they may have had like ones (how pale is one's personal recollection of his own past seasickness or one's present empathy with a friend's), and any removal in time or space washes away all trace of

influence. The emotions experienced by victims of Buchenwald have absolutely no influence on us; the reports of what happened to them do. For these pass through reason and the newer evolved parts of the cerebrum, which are concerned more with group and species preservation. Reason is public and permits co-understanding. A person's private feelings—his appreciation, belief, ecstasy—are of little and rare concern to others, however overwhelming to himself, except as they guide his actions; a person's insights, ideas, thoughts, can be public and can generate co-understanding across centuries and hemispheres. Art is concerned mainly with private feeling, science with public thinking.

Which statement raises sharply the question of whether science can ever penetrate the private or is limited to public "pointer readings." Bridgman, especially, has emphasized that science cannot penetrate the secrets of the individual but must be content with unraveling the class. In the case of sentient individuals, there is the similar problem of subjective and objective, but it will be convenient to consider this separately. There is much validity to this position, but I am not so much impressed with it today as earlier.

For one thing, while it is true that science (as contrasted with art) is rarely interested in the individual as such, it can disentangle the particular instance, with effort, to a degree set by the general level of understanding. Science has revealed enormous detail about our single, individual, private solar system. The physician diagnoses and treats each particular case, as an individual, on the basis of general scientific knowledge. For each person is not merely a member of one class but of innumerable ones—classes of age, sex, weight, race, build, metabolism, blood pressure, artery hardness, and so on—and the criteria of membership in each of several classes soon suffice to define uniquely the individual. This is entirely familiar at the simpler and more abstract level; for three planes (classes of points) determine a point (an individual in each of the three classes). And, while it is true that the internist still is far from understanding the whole individual who is his patient, the analysis can proceed as far as interest and time and abstract knowledge permit. The psychiatrist, devoting far more time to the single patient, knows far more of him than does the internist. So individual and private a matter as one's dreams is now subsumed under general rules, and dream content can be interpreted, predicted, and even manipulated. And Lowes, dedicating years to the scientific study of Coleridge's unique imaginative processes and products, has been able to trace these in superlative detail. If the philosophic position is finally taken, with Bergson or Kant, that, however completely an entity be analyzed from outside, there remains an unknowable essence of being itself, I would only point out that this would have the same degree of validity for a stone, a crystal, a machine, or a tree as for a mouse, a man, or a symphony.

A second weakness in the public-private dichotomy is revealed by consideration of the system to which something is public or private. The sharp margin then melts into a series of gradations. I must refer to other essays for consideration of the nature of orgs and organisms, single systems composed of subordinated interrelated units, of the relationships between units at different hierarchical levels, and of other undeveloped statements in this article. (See *Science*, **101,** 582, 1945, for references.) What is external to the atom, narrowly considered, may be still internal to the molecule of which the atom is part. Likewise, what is public in relation to a particular cell may be private to the multicellular organism in which it finds its being. Yet, because there is interaction and some sharing of entity between

such parts and wholes, the public and private at each level also partly interpenetrate. What is private to the cell is still in part public to the organism, as what is private to the organism is largely public to the cell. But there are solid biological grounds for recognizing a society, or epiorganism, as an org composed of individuals as its penultimate units, and the same considerations apply as to public and private between the individual man and his society. All members of a culture are to an important degree privy to each other's private existence, as they are to the collective privacy of that particular culture. And, in rapidly diminishing degrees, this is also true for all entities as part of a single universe.

This same consideration applies to the problem of objective-subjective, or body-mind, as does also the discussion of class and individual. The private emotions of a particular person are certainly difficult of study, but they are not inaccessible. Direct evidence for awareness is available to each of us only from his own subjective experience. Nonetheless we are completely satisfied, from the similar behavior of others, that all normal men have a comparable subjective experience. What the range is of detailed differences is less easily decided, but the sciences of mind can make highly probable statements on such matters. We are likewise satisfied that apes, dogs, and other mammals also possess a conscious awareness and guess, from the degree of likeness of their bodies and behaviors to man's, the extent to which theirs is comparable to ours. As attention passes on to less similar entities—the simpler animals, plants, unicellulars, viruses, definitely inanimate objects—we feel less confident in extrapolating as to the amount, kind, or even presence of an attendant consciousness. A similar uncertainty faces us as to psychic aspects of our own constituent cells and organs, of our brains in sleep, coma, or under depressant drugs, and of our collective selves as units in an epiorganism.

My own view is: that every org or system offers both physical and mental aspects; that there is the same sort of continuum (in kind if not degree) of awareness as there is of body from the simplest inanimate system to the most complex living organism; that what is private to the org, viewed from the internal aspect, is recognized as subjective and mental, and, viewed externally, the org is seen as objective and material; that each org is a unit in a more inclusive org or is composed of subordinate orgs or, mostly, is both composed of and helps compose other org units so that subjective and objective interplay from level to level; that, to the extent that a given org is poorly integrated and but little set apart from other systems or the rest of the universe, the distinction between internal and external approaches meaninglessness and subjective awareness tends to vanish; and that material and mental are relativistic terms which involve the relation between the observer and the observed.

This condensed statement demands far more elaboration than is here possible, but a few points must be followed up. If mind and matter are but different aspects of the same entity, it is useless to ask if material events cause mental ones or if the psychic can act on the physical. Man, seeking knowledge of the universe, may find the clues, now from his inner experience, now those from the outer world, more pregnant with suggestions for study. It may be convenient and useful to think of an anesthetic producing unconsciousness or of a neurotic state producing gastric ulcer; but ultimately we will recognize the physical situation in the brain, seen most simply as induced by a chemical agent, of which unconsciousness is the concomitant of greatest interest; we will recognize the physical part of the situation, most interesting in its psychologi-

cal aspect of neurosis, which generates another situation, of most concern in its physical aspect as ulcer. In comparable manner, the relation between successive states of a system is sometimes most easily grasped in terms of the first as the *cause* of the second, sometimes, of the second as the *purpose* of the first. Either may suggest the direction in which to seek for the mechanism of the relationship.

This leads to a second critical issue, that of fate or freedom, of inevitability or contingency. On the physical plane, indeterminacy at the electron level seems now established. Whether this is a manifestation of ultimate contingency in nature or is a consequence of our present limits of analysis of the subelectronic realm is perhaps not known. Let us assume that contingency is inherent in systems. Then a system may pass from state A, fully delineated, to state B, slightly fuzzy—only slightly, since certainly what indeterminacy may exist is only marginal. This allows complete freedom of locus for cosmic unrolling over its eternities—as the most minute deviation from geometric straightness will permit a line to curve on itself in a sufficient distance. But it allows, on the usual temporal scale of human interest, a narrow cone of indeterminism, indeed, along "time's arrow." The degree of uncertainty might be more like that in the acting of a play as compared to the certainty of unreeling of a motion picture. Further, this contingency, being uncaused, cannot be a matter of active choice; and certainly cannot be interpreted as the control of the physical by the mental. Our experience of freedom and volition, to come sharply to the human level, is but a part of our experience of awareness: it may be, to be sure, the subjective aspect of whatever amount of contingency is possible to so elaborate a system; it cannot be an uncaused cause of physical action.

Much direct evidence supports this conclusion. Most of man's problem-solving activity is actually unconscious and so certainly not directed by a conscious will. Under posthypnotic suggestion, a person will perform some unusual act—known beforehand to all present but himself—and will be completely persuaded that he acted of his "own free will." Conversely, the compulsive, despite every effort of his conscious will, performs acts of which he disapproves —such as unending repetition of washing his hands. Recently it has been discovered that, during recovery from a chemically-induced coma, a person may "will" to clench a hand, in response to a request, but nothing happens. A minute or more later, while the subject may be busy "willing" to move a foot, the hand suddenly clenches—to the complete surprise of the subject! The observant friend, only less than the trained psychiatrist, can predict with impressive accuracy how a person will decide and what he will do in a great variety of situations. And so on and on through the familiar phenomena and arguments.

Perhaps man should have been able to deduce electron indeterminacy from his own private feeling of freedom, just as he has had to follow from the overwhelming determinacy of the electron and more molar systems to the overwhelming determinacy of animal and human behavior. Certainly we are closer to the internal view of man's mind than of the electron's "mind" and must exploit this vantage point fully in getting clues as to the direction of analysis in the external dimension. Artists, especially poets and other writers who deal with man's inner life, have contributed greatly in noting and clarifying the phenomena ultimately to be analyzed and synthesized—as the naturalist-observer has supplied the raw factual nuggets which were beaten by the theorists and experimenters into such syntheses as evolution. Subjectively, man recognizes purpose as well as volition, values and ends

as well as strivings and means. These clues to nature have been used widely, if not always wisely, in scientific, especially biological, thinking. When the mischievous confusion of personified willing or purposing as a cause of material happenings is avoided this has been very profitable.

The physiologist von Brücke once said, "Teleology is a lady without whom no biologist can live. Yet he is ashamed to show himself with her in public." To discover the purpose or significance or utility of a biological process or structure contributes to an understanding of it just as does the discovery of the mechanism which mediates it. The former suggests directions in which to look for mechanisms and for new phenomena, but it does not lead to that detailed mastery of a problem which enables man to manipulate its course. This comes only from the knowledge of mechanism. Teleology is suspect because, too often, a solution in terms of purpose has been the end rather than the start of an inquiry. The development of egg to embryo to adult—the multiplicity of events: cell growth, division, migration, differentiation; the structural formation of folds, bubbles, buds, and the functional eruption of contraction, secretion, digestion; all in amazingly timed and interacting sequence—all this has meaning in terms of the end result. The outer cell layer folds into a tube *in order to* form a nervous system; the middle layer, to form a heart and blood vessels. The skin over the growing retina forms a transparent lens in order to create a useful eye. Or, a related basis for giving meaning, in terms of history rather than purpose, the gill slits appear briefly in the human embryo and then vanish because the young individual is recapitulating the evolutionary stages of its kind.

Without such orientations the welter of phenomena would be overwhelming; with them, phenomena are put into a pattern, take on form and relative importance, and can be grappled. The major zigs in the zigzag course of science result from shifts in such orientations. But, it bears repeating, these only furnish the basis for the detailed analysis which eventually reveals mechanisms. A mechanical pressure, due to osmotic swelling, due to metabolic products, due to a rich supply of oxygen, due to its surface position, leads to the folding of the outer embryonic layer which forms the neural tube; the forming retina liberates a specified chemical which diffuses to the surface cells nearby, modifies the protein they form so that the molecules are parallel threads rather than scattered balls, and leads to the production of a transparent lens: such are the answers one seeks to demonstrate by experiments directed toward mechanisms. And only such answers enable one, in time, to tinker intelligently with the works—by increasing osmotic pressure in other known ways, by adding other chemicals to influence protein formation, by the myriad tricks that man has learned from nature. Mechanism adds utility to truth.

Conversely, the finding of well-elaborated mechanisms which supply concrete means to a guessed end gives important support to the validity of the guess. It even may supply some quantitative criteria for a scale of values. When man, proud possessor of an overstuffed brain, calls the brain the most "valuable" organ and himself the highest form of life, he does well to suspect himself of anthropomorphizing. Valuable by what criteria? Animals can at least live without brains, they die without many other organs. But when he discovers, in many animals, that during starvation all organs waste away, are used for food, except the brain and heart, which are fed by the others; that a special reflex device is built into the arteries to the brain so that, within the limits of possible adjustment, the brain is insured its

full blood supply whatever happens elsewhere; that the brain is more elaborately protected from injury than any other organ, even than the growing embryo, for it, like the embryo, is floated in a shock-absorbing fluid and, unlike the heart, is completely encased in bony armour; that success in winning habitats and capturing food parallels the development of the brain more than any other attribute: when man discovers these facts, he can feel distinctly more secure in his value judgment about the brain and so about his position. Not only man's mind, but his body and other animal bodies "value" the brain.

We recognize the staggeringly important purpose of mitosis and gene duplication; we are consequently willing to devote tireless effort to discovering how this duplication is managed. We follow forward in time the consequences (purpose) of gene action, we follow backward the antecedents (cause). In the case of an idea too, a sort of social gene, we are concerned with purpose and also with cause; we may deal with it from the mental aspect if, at present, this is more convenient, because immanent, than from the physical aspect. The scientist, especially, but not uniquely the biologist, thus finds in the outer world the counterparts of his subjective experience of purpose, of value, of volition. If he does not confuse his time dimension to mix mechanism and purpose, or become lost in the levels of orgs and so tangled in the internal-external with its attendant mental-physical; if he uses his subjective awareness, as he does his sensory information, as a clue to the universe and of useful ways to study it; then such "projection" is not only warranted but indispensable.

This leads to our last general problem, the epistemological one of reality and knowledge of it. Man may indeed be, with Henry Adams, "on a sensuous raft adrift in a supersensuous sea." But, if so, he will never know it except that the raft expands indefinitely as science extends his perceptions. As argued elsewhere, scientists and their instruments are sense organs of society; and collective man is certainly sensible to vast territories of the universe beyond those accessible to the individual. Not only the microscope and telescope, which extend spatial vision to the minute and the colossal, and the spectroscope, which extends "color" far beyond the visible spectrum; but also electronic instruments which create or detect radio waves, X-rays, radioactive rays and particles, cosmic rays—or for that matter, the simple compass needle which detects a magnetic field—all inform us of phenomena completely foreign to man's biological senses; they stretch the raft. But no extrasensory avenue to the mind exists, there is no royal or divine road to knowledge. This can be certainly demonstrated, I am satisfied, from the facts of biology and especially of neurophysiology, if one accepts the view suggested earlier as to the relation of the material and mental, or even any view which places brain and mind in some interdependent relationship; and such interdependence can also be demonstrated by unexceptionable facts.

Of course, the resultant of any action upon any system includes the nature of the system acted upon; and the neural and mental events provoked by an experience depend also on the nature of the brain and mind in which they transpire. Man's mind can act upon sensory data only in terms of its own organization; all experience is sieved through the "a priori net of the mind," in Eddington's telling phrase, and Koffka has emphasized that thought itself carries a "tone" of true or false. In material terms, this is to say that the structure and function of the nervous system determine the future adventures of nerve impulses which reach this organ; or, more generally, the consequences of any given action upon

the nervous system depend on its general organization and particular state. The "universal" truths are those which are built into the brain machine and are difficult even to discover and recognize, let alone to analyze in terms of mechanism.

Yet real progress is steadily made. "Generalization," "extrapolation," "closure" as recognition of entity, "insight," "relationship" in "more than" or "nearer than" or even "better than;" all these can be demonstrated in animal as well as human behavior, studied quantitatively by such laboratory techniques as the conditioned reflex, and related, still gropingly, to the physiological anatomy of the brain. Revelation is no different. It can, often enough, be understood objectively, especially in the insane; and, however impelling to the individual, can have no collective weight. Unless cumulative reason is held superior to individual inspiration all of man's knowledge and achievement is imperiled. (This is a far cry from Luther's estimate of reason, "that silly little fool, that Devil's bride, Dame Reason, God's worst enemy." Or is it?)

This is not to say that all questions can be answered. Many can only be approached ever more closely, like comprehension of the individual entity as earlier discussed; and, for many of these, the approach has not, or has barely, begun. Science continues to widen and deepen its generalizations, even to ethical problems; but any answer which is inherently beyond the potentiality of science is also beyond the possibility of reliable collective knowledge. Man, individually or collectively, must often act, must answer questions, with no assurance that his conclusion is correct. Well and good, even incorrect action generates new experience and permits improved understanding. But it is a sign of childhood, in the individual or the culture, to believe one's fancies. The adult attitude is to face our present limitations with courage and our final limitations, as these are established, with renunciation.

ASIDE from the more philosophical problems of the outer limits of science, there are a number of particular areas which have been placed "out of bounds" by some scholars. Such areas as "mind," "society," "history," and "values" have been excluded on the basis of arguments now to be examined. (See Anshen's *Science and Man*, and the recent *Scientific Man vs. Power Politics*, by Morgenthau. A position like my own is taken by Lundberg in *Can Science Save Us?*) One other problem, of whether such areas, though all within the realm of science, are yet themselves incommensurable and doomed to qualitative separation, will not be further pursued here. What has been said earlier indicates my conviction that continuity underlies discontinuity and that the lines separating biological from physical science or sociological from biological are only dotted lines.

MENTAL SCIENCE

Psychology cannot ever become a "science," it is argued, for these reasons: (1) the psychologist, in contrast to the physicist, for example, is internal to his subject; (2) values enter into his subject matter; (3) the phenomena he deals with are subjective; (4) they cannot be measured or quantified; and (5) he cannot avoid a personal bias in handling his material. These same points, especially 1, 2, and 5, are also urged in connection with social problems.

The question of internal and external has already been considered in general, as has that of the private and public avenues to knowledge. Man is indeed relatively more internal to his mind, directly apprehended, than to his body or to his culture, and far more so than to his inanimate surroundings. But it remains solely a matter of degree, in practice as well as principle. For, as often

emphasized, the observer enters into the physical observation as definitely as into the psychological; the doctor is part of the case he handles. Nor is the inwardness of mental phenomena, as such, a barrier to study; but rather the reverse. We understand far more of the properties of mind than we do of the brain mechanisms associated with them just because we are more internal to the mental than the physical aspect; because direct observation of our consciousness yields more fruitful clues than the indirect observation of our brains has yet done.

Another consequence of inwardness, urged especially in the sociological field, is that man necessarily alters the phenomena when he studies them. This, of course, is universally true—Heisenberg's indeterminacy is derived from the fact that the photon man uses to measure an electron's position has of necessity altered the position. It is also true that this influence is greater as the systems studied become more complex and closer to man. The physiologist must continuously guard against his experimental procedures making "abnormal" the performance of a body or organ; the physician is well aware of the factor of suggestibility which alters the action of drugs on a patient; and the psychiatrist may depend entirely on "psychotherapy" to relieve a patient's neurosis. But the psychiatrist nonetheless operates, within closer restrictions yet in a wider area, as does the psychologist producing or manipulating "neuroses" in animals, by turning to profit the ability of the experimenter to disturb the system under study. So, also, the fact that persons given bread pills improve in health or performance can be used to measure very subjective phenomena indeed. In an extensive study of the influence of vitamins on the performance of average factory workers, one group received no pills; a second group, a daily vitamin concentrate; and a third, a similar appearing but inert pill. Groups Two and Three believed they were getting vitamins, and the performance of both rose equally over some months, until almost a third better than Group One! Now, had these people not been bombarded by advertising about vitamins, Group Three (and actually Two, as well) would have shown but little change. I am certain one could use some such objective technique to measure quantitatively, if roughly, the effectiveness of advertising or other forms of promotion in arousing belief.

Conversely, even in the sociological field, the process or results of study do not necessarily alter events significantly. Surely the anthropologist, living a year in a primitive community, does not by his presence distort life there so as to invalidate his observations. Surely the actions of sociologists and economists affect their own society so slowly and feebly that their current observations are immune and even their future projections reasonably safe from such an error. The public opinion polls offer a clear case in point. By learning and revealing the state of public opinion ahead of time, it might certainly have led to a change in opinion when put to the realistic test of a vote. This has been much discussed, yet fairly convincing evidence now shows that no such "bandwagon" effect has, in fact, occurred.

The second issue, of values entering into the studies of man, requires little further comment. Values are shot through the biological sciences, and even through the physical sciences, only less than in those of man. Good and bad, perfect and imperfect, higher and lower, better and worse, useful and useless, successful and unsuccessful, beautiful and ugly, true and false, are common in the vocabulary of the natural sciences and are used in senses akin to, if not identical with, their use in man's affairs. Indeed, in one way at least, values may be more of a problem in the natural sciences because they have sometimes been smuggled in unwit-

tingly, in a form of anthropomorphizing, and are not recognized and handled in a clear fashion. In the human area the value problem at least cannot be suppressed or confused, whether or not it can be answered satisfactorily. But this will be considered later.

Third is the argument of subjectivity: mind, being subjective, cannot be studied by objective science; and, in social relations also, the subjective beliefs and motives, often more critical to any understanding than are the overt actions, remain inaccessible. This again turns out to be a matter of degree as to the facts and of some confusion as to the conception. For, as argued, there is a subjective element in every system, and we are only peculiarly happily situated in the case of man to exploit the clues presented from our own consciousness. Introspection, like any observation or exploration, yields grist for the mill of analysis. But the mental sciences, no more than the others, halt at the stage of the naturalist; and the further analysis is, for them also, at the objective level. It is commonly overlooked, despite the impact of behaviorism, that men judge one another—under study or not—entirely by objective evidence; by what they do, how they look, what they say. It is urged that behavior is no index to the "real" thoughts or feelings of a person; but this is, even now, an overstatement, for successful judgments of this sort are commonly made by sensitive laymen and regularly by expert psychiatrists. And as knowledge grows and techniques develop, the demonstrable correlation becomes ever finer.

The humorous result of testing a lunatic, who regularly claimed he was Napoleon, on the "lie detector" yielded powerful support for this position. Being asked during the test, "Are you Napoleon?" he craftily replied, "No." But the pulse and respiration changes registered a lie. Clearly, in his belief, he *was* Napoleon; and this belief could be objectively established. By study of the bodily concomitants of mental states, by noting behavior under selected conditions and with few alternate possibilities, by pitting one possible satisfaction goal against another, by the rapidly developing technique of attitude scales, personality trait tests, and the like; by all these means and many more certain to be devised, subjective states can be identified and quantified (still only roughly) from the objective aspect. Such evidence likewise refutes the fourth argument; that mental entities—beliefs, motives, desires, ideas, even dreams—are inherently nonquantifiable. They can be and are being measured.

Last is the problem of bias. This is surely a general human failing which manifests itself in all science as well as in all of life. Each man approaches a problem with his particular limited orientation and, having devised a solution which satisfies him, develops some parental affection for his hypothesis. But it is just the strength of science, as a cumulative human effort, that all possible orientations are finally brought into consideration and that individual emotion is countered by other individual emotion until all affect is rubbed from a rational core of common agreement. In proportion as the facts and relations are clearly and unequivocally demonstrable does this core of conviction as to the "truth" emerge easily and rapidly. Surely, then, "proof" is more simply attained in the physical sciences than in the mental or social ones. But this reflects the present state of development and not the impossibility of more. Even now, bias and emotionalism are turned to positive advantage in the psychological realm. The psychoanalyst, noting the regular appearance of an emotional "transference" by the patient to him and a lesser "countertransference" from him to the patient, has deliberately utilized this "bias"

situation to gain insight into, and to direct the cure of, the disturbed psyche of his patient.

SOCIAL SCIENCE

The arguments offered by those who would place social phenomena beyond reach of the scientific grasp include, besides those already considered, such ones as the following: (1) Causative factors are too numerous and complex to be handled. (2) Phenomena are so individualized and variable that it is impossible to classify them meaningfully, which is essential for the attack of science. (3) The objects of study, men, must give their active cooperation; which may not be secured or, if secured, may be colored. (4) Experimentation is impossible, because of moral or social restrictions, the nonrepetitive sweep of history, and the sheer mass and magnitude of the forces and events. These are mainly methodological problems and not so serious, even if valid, as the next group, which is ontological. Thus: (5) Man alone has a history known to the subject of that history. Man's past, therefore, can influence his future not only in the usual sense of a causal sequence, but also by guiding his conscious choices. (6) Man, as the historical argument also implies, is free and his behavior, therefore, indeterminate. (7) Man purposes, creates, and shifts his values and accordingly brings about unpredictable social change; for (8) the individual is prior to society and not the reverse.

So far as the first group of objections is concerned, it is surely obvious that they carry weight only in degree, not kind. Every one of them has been urged in the same vein in the past to prove that there could never be a science of biology. If the position were: social phenomena, being especially complex, individualized, and hard to manipulate, must remain for a longer period, or even indefinitely, less fully analyzed, and so less predictable, than those of the physical world; and that a body of knowledge cannot be called a science until it has achieved some minimum of these desiderata; then the only argument would be about the position of the dividing line and when each aspect of social study is judged to have crossed it. But to argue on these grounds that sociological problems are extrascientific is nonsense.

We have come to accept, by reiteration, that the social is more complex than the biological; but I sometimes wonder if the reverse is not the case. The cell population of a single human brain is four or five times the total human population of the world. These cells are in direct or indirect, and reciprocal, "communication" with one another to a far greater degree than are men—it has been estimated that the permutations of their connections are greater than the total of particles in the known universe. Each cell has its individual role, partly irreplaceable, in the whole, its own quantitative, and often qualitative, irritability pattern, its bizarre structure as a variable unit in one of hundreds of distinctive groups. In number and nexus, if not nuance, relationships in the brain are far more complex than in a social group. But who would try to tell the neuroanatomist or physiologist that his study of the brain is extrascientific or his progress in understanding is illusory? The vast range between a micrococcus and the mammalian brain (let alone the whole mammal), a range perhaps greater than from a salt crystal to the microbe,* is yet effectively bridged by the same basic concepts and by like methods. Is the chasm from solitary to social man as great?

Chemists have found or made over a million separate kinds of molecules of pure substances; and entomologists have identified a similar number of species of insects.

* In sheer mass, a sulphur-bottom whale is as large compared to an influenza virus body as is a large drop of water compared to a single water molecule.

These various individual classes have nonetheless been grouped, analyzed, accounted for, understood to moderate or high degree by scientific procedures. Is the variability greater of a few hundred men in a tribe or village or a few million in a nation? Of course, the men are units in a single class; but so also are the insects and molecules, and, conversely, each man is also a class of organs and cells. And these human units do not vary over a wider range in mental or social attributes than they do in physical—if the studies of psychologists, psychiatrists, and social anthropologists mean anything, these are perfectly measurable and classifiable. Would "Middletown" be of significance to anyone, except residents and their friends, if it were not representative of a class? Would even "Main Street" interest us if we didn't recognize the class of Babbitts?

Nor is the need of obtaining cooperation of subjects either unique or entirely disadvantageous in the social field. Just as the observer enters into all observations, so does the "submission" of the observed. If a substance "refuses" to melt sharply but chars instead, its melting point cannot be determined. Or, more clearly comparable, a dog must cooperate even when being given an injection, at least to the extent of not biting or struggling. He can, of course, be compelled, by force or fear, which might suffice for injecting a narcotic; but if studies on the sentient animal are at issue, the cooperation must be positive. Only an animal gently and carefully trained to lie quietly can be used for basal metabolism studies, for full relaxation is essential. A similarly trained dog, willing moreover to swallow a balloon and tube, is needed to observe stomach movements, for any emotional arousal stops them dead. And animals used in conditioned-reflex studies are "cases" almost in the sense that are humans: their full history is desirable, their daily life is noted and controlled, they are elaborately educated, they often are incorporated into the family of the investigator, they may react differently in the presence of different experimenters, and so on. They cooperate at somewhat the same level as does a child in school or taking a mental test; or as an adult after once volunteering for some experiment. Actually, since a human more readily understands and *can* cooperate, some conditioned-reflex studies are made with greater ease and certainty on man.

The same general incentives to cooperation, those of reward or punishment, are active with man and animals and have been shown to operate in the same manner. At the simple level of food versus shock, conditioning proceeds alike with man or dog, and at the more abstract level of giving praise or blame (by word and intonation)—indicating, that is, value judgments—training again follows parallel courses. Conversely, humans may be exposed to desired conditions, or observed under them, with no active cooperation on their part. It is a similar objective problem in each case to note which individuals sicken during an influenza epidemic, which blush at a taboo word, which choose green ink for their correspondence, which vote at local elections, or go to church or belong to the CIO, which falsified their income tax returns, even which say they believe in God or internationalism or free trade. It is little different when a children's play group is observed through one-way mirrors and with microphones. And just because man can react, by act and word, in finer detail than can animals, his cooperation as a subject is mostly an advantage, not a handicap.

Experiments in the sociological field can be, and are being, performed, though the techniques are only now being developed. It is a sociobiological experiment when a trace of fluorine is added to the drinking water of one small town over a period of years, while

a neighboring one is kept as control, and the amount of tooth decay in both communities noted. It is pure sociological experimentation when diet education is given in the grade schools of some sectors of a backward community and not in others, and the change or absence of change in food habits noted. So also for studies on racial cooperation in collective farming groups in the "white-trash" zone which, like the above cases, can be carried out with sufficient numbers of experimental and control units to be reasonably decisive for the factor under study. Even a single experiment like TVA, while not entirely convincing until like findings are obtained in additional instances, may carry a high probability of significance. Experiments have amply shown that youth groups flourish, in all ways measured, under a democratic leader as compared with an autocratic one. Evaluation studies underway should objectively demonstrate strengths and weaknesses of alternate education procedures. Market analyses, radio listener ratings, promotion studies, opinion polls, are all techniques for measurement which, combined with experimental manipulation of the product or program or advertising or publicity, are yielding reliable and predictive generalizations, which will in time transcend their applied focus, in the area of human affairs.

The ontological arguments have been met in the general considerations presented earlier, but a few further comments are desirable here. Assuming, as above, some ultimate contingency in nature and also the inner-outer view of mental and material, there still remains neither a basis for regarding man's will as an independent active agent nor any fulcrum on which a mental state can lever a physical one. And the degree of indeterminacy of any system over a time span commensurate with its duration is minute at most. These points have much bearing on the problems of human history; of whether history, being known, diverts the future unfolding of human actions and their social residues, and whether historical study can itself be included in science as "scientific history."

The argument as to whether history can be a science has raged for a century. Frank, in his recent *Fate and Freedom*, emphatically decides not; on the basis of the fugitive character of what "really" happened, the selective bias of the historian, the accidents of men and events which laugh at predictive rules, the inherent evitableness rather than inevitableness of human affairs, and all the other general objections presented in the sociological area. These are considered more fully in last year's *Ethics*, but it deserves note that the points here mentioned all apply with equal or greater force to archeology and paleontology and to much of ethnology and social anthropology—all universally accepted as in the scientific fold. The whole evolutionary past of the world of life is, of course, a historical study, a description of what was, with all the dangers of vanished evidence, theoretical blinders, the riot of chance, and fuzz of contingency; but, also, following description came analysis of guiding factors and forces which, today, are being resolved into quantitative formulae (Wright) and are yielding verifiable predictions. The role of new genes in biological evolution and of new ideas—mutations both—in social evolution is comparable even though the mechanisms are different enough to operate respectively over aeons or decades.

Conversely, the fact that man knows or can know his history, like the comparable facts that he knows his present mind, individual or collective, and his future aims, does not vitiate the degree of determinacy that exists in the cosmos. Man's mind (and brain) evolved without a "purposing" by man; it made possible a complex society, a knowable past, and an envisioned future; it is the internal aspect of reality and does

not independently operate upon the external aspect of the same reality. The significant problem is that of interaction of part and whole, of organism and epiorganism (as of cell and organism in biology); and of those regular temporal sequences in state of all orgs (systems) in which we describe mechanism and see cause or purpose.

Man's conscious purposing, on this view, is part of the scheme of cosmic change. It is our internal view of the succession of events, looking downstream in time rather than up. Purpose, even retaining its anthropomorphized connotation of the purposer as a free and independent mover, is constantly referred to other men and to most animals. But the same objective picture remains when any conscious aspect is ignored and systems are viewed externally; we see purpose or teleology when the behavior of an org is more intelligible to us in the light of its future states than of its past ones. This is sometimes the case for inanimate systems, as when "water *seeks* the lowest level;" it is commonly true for animate ones. The organism, as unit or as species, continuously solves problems; and it changes its "values" of a good solution. All evolution is a succession of solved problems—of successful reproduction, of capturing food, of rapid movement, of survival in dry or cold surroundings, etc.; and of the secondary and tertiary problems which arise in meeting the primary one—and the relative "goodness" of solutions changes with environmental conditions. A species that has been beautifully adapted to, and successful in, one environment is quickly branded unfit when this changes; as the exiled St. Petersburg aristocracy was in Paris.

All structural development and physiological activity of individuals is a similar succession of solved problems, with shifting values. Blood is shunted to the muscles when one runs, to the gut during digestion; to the skin "to nourish" it, yet from the skin, which may be sacrificed, "to conserve" body heat. A hand is jerked reflexly from a hot bar "to avoid" injury, but it will grasp the same object reflexly "to maintain" balance. Almost always the physiologist first sees meaning in phenomena in terms of such purposes; as analysis proceeds he discovers new phenomena which enable him to think upstream in time, in terms of mechanisms. There is no reason to view man's purposing differently, it is only more difficult to emerge from the immediate internal view of it.

And so for the argument that society is merely the outcome of many individual ends. Part and whole act on each other reciprocally—such coeval, rather than sequential, interaction is part of the very character of orgs—and neither is truly prior to the other. The several aims of organs as parts of a man, of a single person as an entity, of men as units in a society, of a single social epiorganism, are in continued interplay. There is at each level and between levels conflicting and compatible purposes, competition and cooperation. Only while the centripetal forces exceed the centrifugal ones does the org continue as such; otherwise the liquid evaporates, the body dies, the group disintegrates. Infants form no group, several together go their separate ways which may cross in conflict but not in joint play. With development, cooperation increases, absolutely and relative to conflict. In evolution, the early stages of many types are solitary, the more developed ones social. And man's history likewise is a record of org evolution toward cooperation of the parts and integration of the whole.

Of course, what has been said of purpose applies to values. I see no reason why value problems are extrascientific. Psychologists study animal learning and actions in terms of "goal-directed" behavior, biologists recognize values in body organization, function, and evolution, psychiatrists expose

unconscious drives which underlie conscious purposes. All see ends and seek mechanisms. All find, with increasing success, factors in the organization, history, and environment of individuals or types which account for their particular values; all can trace values, more or less precisely, even quantitatively, back to biologic "needs" and forward to predicted actions. As Perry has argued, values are facts. Objects of value are objects of interest, and interests of value are interests in objects (whether of present or future or imagined existence), so that no real separation exists here between objective and subjective values. And knowledge concerning the relations of nonexistential goals, ideals, and the like is in the same category of knowledge as mathematics, which also deals with the relations of nonexistent entities.

One stock objection to a science of values is that "ought" is beyond science. Yet this word, with all except the volitional connotation, is continuously used outside the sector of human actions. And, more weighty, the very notion of "ought," like that of "must" or "true" or "cause," is implicit in the functioning of brain and mind. The whole process of conditioning shows, in animals as clearly as in man, the unverbalized and unthought axiom: past regularity *ought* to extend into the future.

Another objection is that reason cannot be the ultimate standard, for it can ask the question whether it is ultimate. To the extent that this has any real meaning, two answers are relevant. First, that if science and reason must forswear absolute certainty, even more so must belief, faith, and revelation. Second, and regularly overlooked, the collective mind of man (or the mind of collective man, of the epiorganism) transcends the mind of individual man. The cumulative knowledge, imagination, and critical reason of mankind is of a different order than that of the ablest person. Mankind's wisdom is surely finite when projected against the teeming universe, but no less is it infinite as measured against a single man. And, in the same vein: as man's science, his accumulated and organized and tested knowledge, leads him to state evermore-sweeping generalizations, with ever more confidence in their enduring validity, about what the world is like and in which way it is moving, he approaches closer to an empirical answer to the grand problems of philosophy. Certainty of absolute values we shall never have, but certainly science will supply values founded on an immense knowledge of the universe, wide in space and deep in time, values more weighty to all men than those imposed by a particular period and locale on some of them.

ACID CLAY—A WEATHERING AGENT

By W. D. KELLER

Department of Geology, University of Missouri

THE minerals which serve the most critical needs of man and which merit a rank of most fundamental importance to the human race even in this atomic age are not the ores of iron and gold, nor coal, platinum, uranium, or radium, but the clays. Clay minerals, those colloidal, molecular, microscopically elusive, versatile, and adaptable, but yet crystalline, plates and needles which act as "jobbers" between the inorganic, lifeless rocks and the life-possessing and life-sustaining food plants, become the foundation stones that support the entire vital structure culminating (that is, "culminating" from man's viewpoint) in man. If driven to the extremity of Adam or Pithecanthropus, we could do without other minerals but we would need clays to grow the food and fuel to sustain the race, hydroponics notwithstanding.

Because of our dependence on clays and because they have been with us since before "Adam was formed of the dust of the ground," it is reasonable to expect that researches on them would have long been so thorough and exhaustive that little of major importance would remain to be discovered about their nature and origin. Quite the contrary is the case, however. Only a little while before World War II, a new agent in the chemical weathering of rocks was described by Dr. E. R. Graham, of the Soils Department of the University of Missouri, who recognized that clay minerals themselves might play an important role in decomposing the primary silicates into more clay minerals, and so on and on. Prior to this time the action of ground water with carbon dioxide, oxygen, and various indefinite humic acids was held chiefly responsible for the alteration of silicates like the feldspars, amphiboles, and pyroxenes into clay. It is interesting that Dr. Graham's contribution stemmed from activities in two scientific fields. By means of an alert imagination, he coupled a rich background of soil science with a geological perspective to erect the concept and then proved experimentally that acid clay functions as an active aggressive agent in the weathering of rock minerals.

Fundamentally, the surficial decomposition of most silicates like the feldspars and mafic silicates is a result of hydrolysis of the aluminosilicates. The primary minerals, exemplified by orthoclase, a potassium-aluminosilicate, are salts of weak aluminosilicic acid combined with strong base-forming elements such as potassium, sodium, calcium, or magnesium. When the aluminosilicates are wetted, hydrolytic breakdown occurs, the pH of the solution rises, mobile alkali dissolves, and a hydrous aluminosilicate, relatively insoluble, remains. Obviously, a removal of the basic alkali or an introduction of acid into the system will upset equilibrium and promote more hydrolytic decomposition of the primary silicate and the formation of more new clay. Therefore, the carbonic acid arising from the solution of carbon dioxide derived from the air or the soil atmosphere, and the organic acids from plants and plant residues, act to increase hydrolysis.

Likewise, any clay having acid properties, like that from an acid soil, reacts with the products of hydrolysis to drive the reaction toward the decomposition side. Consider the magnitude of acid soil clay available right within the critical locale of rock weathering, and one can appreciate the significance of this agent. Immeasurably large areas of primary minerals and rocks, exposed as surfaces of innumerable silt- and

sand-sized particles occurring in the soil, subsoil, and mantle rock to the wide surface of underlying solid rock, are susceptible to the gentle but ever-pressing corrosive attack of acid clay, which may coat the primary material most intimately.

A single neutralization of the once acid clay is not the end of its action. The base-forming cation (potassium, sodium, calcium, or magnesium) is held only loosely to moderately tenaciously by the clay's space lattice and may be removed by repeated ground-water leaching with simultaneous replacement by a hydrogen ion. The further this replacement proceeds, the more acid the clay again becomes and the more aggressive it is toward further weathering.

Of still greater significance is the removal of the metallic cation by plants utilizing the mineral nutrient in their growth. Plant nutrition (of mineral nutrients), as we know it today, involves predominantly the extraction of mineral nutrients loosely held by clay minerals and organic matter. The more vigorous the growing crop is, the more quickly calcium, potassium, etc., will be taken from the clay and hydrogen substituted for it, and the more quickly it will become acid. In consequence, the recurring acidity of the clay rearms it with power to steal away calcium, potassium, etc., from more primary silt minerals by decomposing them and releasing more new clay. That is one of nature's ways of attempting to restore the mineral nutrient balance in the inorganic-organic cycle. Indeed, the clay minerals are "go-betweens" for the inorganic and organic worlds.

Graham found (1939) that within 107 days an acid clay colloid having originally a pH of 3.3 reacted with silt-sized anorthite (a calcium aluminosilicate feldspar containing probably a little sodium) particles to raise the pH of the solution to 5.7. Moreover, within those 107 days, 3.4 percent of the total calcium of the anorthite had been taken into solution. By extrapolating those data, a somewhat speculative extension, we might expect silt particles of anorthite to be competely (100 percent) weathered in a highly acid soil within 10 years. Obviously, other modifying factors, such as temperature, leaching by rainfall, plant growth, and activity of carbonic and humic acids, would affect the rate of decomposition, so the figure of 10 years, with the implication of anorthite impoverishment of soil in 10 years, must not be taken too rigorously. It does indicate, however, an approach to one line of attack on the formation and replenishment of soil and on rock weathering.

The element iron, which is present in the amphiboles and pyroxenes, was left out of the above discussion because it usually oxidizes and hydrates, probably more or less independently of silicate hydrolysis, in the weathering environment. Where the primary mineral is essentially an iron or iron-magnesium silicate poor in aluminum, the acid clay will probably be as corrosive as with an aluminosilicate.

As a weathering agent the unobtrusive acid clay may be likened to a bloodsucking, sapping, parasitic guest which eventually fully coats and encloses its rock or mineral host. Slowly, but with almost irresistible sureness, this parasitic agent pulls out from the host its metallic atoms which link its silicate sheets and hands over those elements to plant roots, leaching water, or other chemical solvents. The predatory guest effects insidious alteration, with release of energy, of the host mineral to clay of its own family and continues to bore in. Acid clay is indeed a powerful weathering agent that has worked in a hidden fashion for a long time. We have reason to be thankful that acid clay has been restocking the soil and mantle rock with its own kind of mineral through long geologic time, and that it has been a medium of transfer of mineral nutrients from stones to wheat and corn and bread and meat.

SCIENTISTS AND THE CIVIL SERVICE*

By ARTHUR S. FLEMMING

United States Civil Service Commission, Washington

THE recruitment and retention in the federal service of the nation's best-qualified scientists is one of the government's most serious problems. Complex and tremendously significant duties and responsibilities have been given to federal scientists, and the welfare of the nation, if not the welfare of the whole world, calls for the discharge of these duties by persons of the highest competence. The federal government is aware of this fact, and, as a result, it is engaged in the development of a program designed to attract to, and retain in, the federal service our very best scientists.

As the central personnel agency of the federal government, the Civil Service Commission must, of course, assume a position of leadership in the development of such a program. This we are doing. We are doing it, however, with the active day-by-day assistance of an Advisory Committee on Scientific Personnel. In view of the important role which this committee has played in the development of our program to date, and in view of the important role it is sure to play in its further development we desire at the outset to identify the committee and to pay tribute to the job which it has done and is doing.

From the beginning, the Chairman of this committee has been Dr. M. H. Trytten, who is the Director of the Office of Scientific Personnel of the National Research Council. Dr. Trytten's leadership had done more than anything else to insure the effective functioning of this Advisory Committee. Associated with Dr. Trytten as members of the committee are Dr. Thomas B. Nolan, Assistant Director of the Geological Survey, Department of the Interior; Dr. Edward U. Condon, Director of the National Bureau of Standards, Department of Commerce; Dr. S. B. Fracker, Assistant Director, Agricultural Research Administration, Department of Agriculture; Captain Ralph D. Bennett, Technical Director, Naval Ordnance Laboratory, Navy Department; Dr. Lucius F. Badger, Assistant Director, National Institute of Health, Federal Security Agency; and Dr. Kenneth L. Heaton, Office of Director of Civilian Personnel, War Department.

Now let us take a look at the program itself. Prior to World War II, recruiting for the federal government was a highly centralized operation. This was particularly true of recruiting for professional and scientific positions. A considerable portion of the work involved was centralized in the Civil Service Commission. As far as the Commission was concerned, a large part of the operation was centralized in Washington. Generally speaking, there was a conviction shared by people both in and out of government that this was the only way in which a system of open competition could be effectively administered.

The reasons for such a belief were clear to all concerned. The civil service system was brought into being in order to fight a spoils system. A reluctant Congress brought it into existence because of an insistent demand upon the part of the public. A hostile group of operating officials was determined to prove that a merit system just could not work. Consequently, surrounded as they were on all sides by enemies of the merit system, the first

* From a speech before the Annual Meeting, Middle States Science Teachers Association, New York, November 30, 1946.

civil service commissioners decided that it was imperative to keep a close check on the handling of each individual case. Centralized controls, in their judgment, were absolutely essential to the survival of anything resembling a merit system. Unquestionably, this nation is indebted to those who held in a determined manner to such a concept. They are the persons who are responsible for the deep roots which the merit system now has in the federal government. They are the persons who are responsible for the fact that some of the nation's most outstanding scientists have been able to work, year in and year out, in the federal government's laboratories, irrespective of their political affiliations.

With the advent of World War II, it became clear that centralization just would not work. The reasons for such a conviction were obvious. Centralization resulted in delays. Delays of months and even years in establishing lists of eligibles were cited in support of this point of view. And many times, even after long delays, it was felt that the best-qualified available personnel had not been secured. Also, centralization of recruiting kept those, for example, who were responsible for supervising the government's scientific projects from playing an active role in the selection of their personnel. In other words, centralization violated a basic principle of sound management.

Throughout the war period, recruitment for the federal service, particularly in the professional and scientific fields, was characterized by a high degree of decentralization. The Civil Service Commission continued to assume responsibility for the establishment of qualification standards. It also assumed responsibility for seeing to it that there was adherence to these standards. But the departments and agencies, in many instances, assumed direct responsibility, with the cooperation of the Civil Service Commission, for the recruitment of their own personnel. As a result, there was a genuine pooling of the resources of the federal government.

With the war over, the Commission was faced with the necessity of determining to what extent it was going to continue to operate under a policy of decentralization, and to what extent it was going to return to a policy of centralized controls. The decision has been made. And the effect of that decision is to carry decentralization even further than it was carried during the war.

Here is how this decision is being carried out in the recruitment field, with particular reference to recruitment for professional and scientific positions:

1. The departments and agencies of the federal government have been invited to establish United States Civil Service Boards of Examiners within their departments.
2. These boards are to be made up primarily of top operating officials and outstanding specialists in subject-matter fields. Nominations for membership on the boards must be approved by the Civil Service Commission.
3. The boards are to be established both in Washington and in the field service.
4. These boards are to develop, subject to the approval of the Commission, minimum qualification requirements for positions for which they hold examinations.
5. Also, these boards, in cooperation with the Commission, are to conduct active recruiting programs designed to attract persons of outstanding qualifications.
6. The members of the boards are to be responsible for rating the applications received for examinations, and are likewise to be responsible for establishing eligible lists and certifying names from these lists for the filling of vacancies. These actions will all be subject to review by the Commission.
7. Boards of Examiners will hold examinations for positions which are peculiar to a department or agency. Where a position is peculiar to a comparatively few agencies, a joint Board of Examiners will be established. Where a position is common to all departments and agencies, the examination will ordinarily be conducted through the Civil Service Commission's own offices. Even under such circumstances, however, if the department or agency is prepared to carry forward a highly specialized recruiting

program of its own, the Commission will usually, in the professional and scientific categories, be willing to have such a program handled by a Board of Examiners.

This is not a paper program. These boards are already in the process of being established. By January 1, 1947, over 750 of the boards were in actual operation.

During the fiscal year ending June 30, 1947, we expect to make approximately 600,000 regular civil service appointments in all occupational categories. Over 300,000 of these appointments will be made from lists of eligibles established by these Boards of Examiners.

The figures in the professional and scientific fields are even more significant. In strictly professional and scientific jobs the federal government will be called upon to make over 33,000 regular civil service appointments during the same fiscal year. If we can find these 33,000, over 22,000 will be made from lists of eligibles established by Boards of Examiners made up of outstanding professional and scientific men in the various departments and agencies.

In operating under this policy of decentralization, the Civil Service Commission is not divesting itself of responsibility for what happens in the field of recruitment. This we could not do under the law. This we could not do and preserve a genuine civil service system. We are saying, however, that we shall discharge our responsibility by (*a*) setting standards, (*b*) delegating authority to others to act within those standards, and (*c*) checking up from time to time to make sure that there is adherence to standards.

On the whole, departments and agencies are following a similar policy. During the war, they delegated authority to act on personnel matters to their field establishments. They found that it worked. Consequently, in most instances, they are continuing this policy of decentralization. In this manner, both the Civil Service Commission and the departments and agencies are making it possible for professional and scientific personnel to participate directly in recruiting activities. In this way, also, the government's recruiting machinery is being greatly speeded up.

All this has definite implications from the standpoint of recruiting for the public service some of the best scientific students in our institutions of learning. It means, for example, that the responsibility for keeping in touch with the colleges and universities as the source of supply for professional and scientific positions in the federal service will be a joint responsibility resting upon the Commission and the departments and agencies. It means that, in addition to personnel officials, those in charge of our scientific laboratories will also take a definite interest in making appropriate contacts with colleges and universities. They will manifest this interest because they themselves will be playing a definite part in recruitment activities. It also means that there will be increasing emphasis on contacts between the field offices of the Commission and operating agencies and the colleges and universities.

That all this is not idle speculation is shown by a very recent development at the grass-roots. The Civil Service Commission has a regional office at Denver, Colo., which serves federal establishments in the states of Colorado, Utah, Wyoming, and New Mexico. In that region there has been organized a College-Federal Service Council. Operating heads of the principal federal agencies in the Denver region and administrative officers of colleges and universities located in that region are members of the council. The council has now provided for the establishment of committees on the social sciences, physical sciences, and biological sciences. These committees are made up of representatives both of federal agencies and of colleges and universities.

This College-Federal Service Council will work on the problem of providing a better flow of qualified persons from the colleges and universities into the federal service and will likewise work on the problem of making the facilities of the colleges and universities available for the in-service training of persons already employed by the federal government.

The committee on the physical sciences is now obtaining factual information from college professors, agency heads, personnel officers, and federal employees which it intends to use as a basis for findings on the requirements and policies of the federal service with respect to collegiate training, recruitment, in-service training, and advancement of personnel.

The Commission has followed this project with great interest. It is convinced of its worth-whileness, and as a result, similar developments will take place in all our regional divisions. In addition, steps will undoubtedly be taken in the direction of setting up a similar group at the national level.

In the development of this program for the utilization of Boards of Examiners, particularly in the professional and scientific fields, we have had the benefit of the continuing assistance of our Advisory Committee on Scientific Personnel.

THE Civil Service Commission realizes, of course, the importance of qualification standards in the development of an effective recruitment program. If our standards are high, it helps to attract outstanding persons. If they are low, such persons show very little interest in the possibilities of federal employment. Our Advisory Committee is likewise very much interested in this phase of the problem.

The Commission is at present engaged in a project which is sure to have far-reaching implications as far as this question of standards is concerned. We are in the process of developing what we refer to as classification standards. These standards are written statements outlining the duties and responsibilities of various classes of jobs and indicating the series and grades to which these various classes should be assigned.

These statements are very important from a salary point of view. This is true because the grade in which a particular position is placed determines the salary the incumbent of the position is to receive. In addition, these standards contain a statement of the knowledge and abilities required to perform the duties of particular classes of positions. They are not statements of the number or types of courses which must have been completed in order to qualify for such positions. They are statements of the knowledge and abilities which a person must have to perform the duties of the position, irrespective of how he may have acquired such knowledge and developed such abilities.

The relationship between such fundamental statements and the development of courses of study in the scientific field is, of course, obvious. With such statements in front of them, the members of the physical and biological committees of the College-Federal Service Council in Denver, for example, will be able to develop a much more realistic program than they could without such statements.

When the Advisory Committee on Scientific Personnel was acquainted with this program, it recognized at once its importance from a long-term point of view. Consequently, members of the committee are participating actively in the development of the standards for scientific positions. We feel confident that with their help we shall be able to develop standards that will attract our best minds to the federal service.

And so, in a variety of ways, the federal government is putting itself in a position

where it can carry forward a more effective recruitment program in the scientific field. This alone, however, will not solve this problem of attracting to, and holding in, the federal service the best-qualified scientists the nation can produce. Conditions surrounding employment within the federal career service must be of such a nature as to appeal to men and women who have decided to devote their lives to scientific pursuits.

Considerable progress has been made in this direction. Our salary schedules in the middle and upper brackets compare very favorably with those in colleges and universities, as well as with those in industry. The working conditions surrounding government employment more and more approximate those of the most progressive employers in the nation. Government today is on a five-day week. Federal employees are entitled to more than five calendar weeks of annual leave each year. Sick-leave provisions are liberal. In addition, the federal government has developed a retirement system which, from the standpoint of professional and scientific workers, is an attractive one and again compares very favorably with retirement systems outside the government.

Recognizing that progress has been made, we of course realize that much more remains to be done. Here, for example, are a few things that we feel must be done in the months which lie immediately ahead if we are to attract to, and hold in, the federal service first-rate men and women.

1. We must pay higher salaries to our top professional, scientific, and administrative personnel. Our present ceiling of $10,000 on salaries for the career service should be lifted to at least $15,000.
2. We must provide better opportunities for continued growth to those who are engaged in professional, scientific, and technical work. In-service training and leaves of absence for advanced study, as well as opportunities to observe how problems are handled outside of government, should all be included in such a program.
3. We must develop programs for promoting career servants which will minimize the possibility of such persons finding themselves in dead-end jobs.
4. We must do much more than we have done up to the present time in the development of administrators who are skilled in the handling of professional and scientific projects and who know how to supervise those who have dedicated their lives to the pursuit of truth in the scientific field.

Up to now, we have been talking about things that the federal government is doing, and the things which the federal government recognizes it must do, if it is to attract and hold in the federal service our best-qualified scientists. But if this goal is going to be reached, we must receive a great deal more help and assistance from the faculties of our institutions of learning than we have received up to the present time. Generally speaking, students of science have a woeful lack of understanding as to the manner in which the federal government, as an institution, actually functions. They do not know what the opportunities are for the carrying forward of scientific work in the federal government. They do not know what policies the federal government, as an employer, actually pursues.

There is no excuse for this kind of situation. Whether these students of the sciences are to become government employees, or whether they are to go back to their communities and assume their responsibilities as citizens, it is incumbent upon us to give them, while they are students in our institutions of learning, a much better understanding of our federal government. If we do, some of the best will decide to become career public servants.

Those who do not will go back to their communities and provide information as to the progress that has been made in the development of a career public service

in the federal government. They will also know what needs to be done in order to strengthen it. In other words, they will help to place back of the career public service an informed public opinion without which it is impossible to attract to, and hold in, the public service our best-qualified citizens.

We are going to hear more and more about the federal government's needs in the scientific field. We are going to have more and more contact with the top scientists in our federal scientific laboratories. Let us approach our problems with an open mind and become intelligent citizens as far as our government is concerned. Insist that the students in our schools likewise become intelligent citizens as far as the government is concerned. Then, if we who are a part of the public service will keep working at the job of making government an increasingly attractive employer for scientists, together we can make sure that our nation's laboratories are supplied with scientists who are second to none.

BUTTERFLIES

Gauze-winged, they flutter, one by one,
These bright ambassadors of sun,
Brushing past each leafy bower,
Appraising sweetness of the flower.

What radar guides their aimless flight
Through devious avenues of light
Until unerringly they rest
Upon the clover's nectared breast?

MAE WINKLER GOODMAN

PERCEPTUAL RESEARCH AND METHODS OF LEARNING*

By G. T. BUSWELL

Department of Education, The University of Chicago

RESEARCH in visual perception has depended largely on two sources for data. One of these sources has been the studies which have used various types of tachistoscopic apparatus; the other has been studies of eye movements using photographic or other kinds of recording techniques. My purpose is to discuss the applicability of this type of research to methods of learning, with particular emphasis on learning to read.

The literature of psychology includes a long series of tachistoscopic studies, ranging from those using such simple devices as flash cards to those employing precision tachistoscopes by which duration of exposure can be controlled to a thousandth of a second, and dealing with various kinds of subject matter such as numbers, geometric forms, and verbal materials. The purpose of these experiments has been to improve accuracy and scope of perception and to reduce perception time. Recent application to military problems has focused increased attention on this kind of research. Renshaw's work, reported in the June 1945 issue of the *Journal of Psychology* and in other places, has become widely known. Less spectacular, but ranging over a longer period of time, have been the experiments aiming at an improvement of the reading process by use of this method. The flash card, which is in essence a poorly controlled tachistoscope, has long been used as an aid in learning to read; variants of the tachistoscope, such as the American Optical Company's Metronoscope, the kinetoscopic apparatus used with the Harvard reading films, and the film-projection apparatus used in the University of Chicago reading laboratories, have been employed in clinical work in reading at all levels of maturity.

Paralleling this use of tachistoscopic methods has been the development of techniques for studying perception by the use of various devices for recording eye movements. These range from the first crude attempts of Javal to secure records on a smoked drum by means of a mechanical lever attached to the cornea of the eye to more elaborate types of apparatus for photographing eye movements developed particularly by Dodge, Judd, and Dearborn. More recently eye-movement data have been secured by electrical recording of muscle currents. These eye-movement studies have dealt with a wide range of problems involving the perception of numbers, verbal materials, and pictures. They have provided data on continuous perceptual behavior as contrasted with the discrete, separate responses recorded by the tachistoscope.

The implications of tachistoscopic research and eye-movement research are understood better when considered together. When considered alone extravagant hypotheses can be drawn. For example, it is easily possible to learn to recognize words in a tachistoscopic exposure of one-hundredth of a second. However, data from eye-movement studies show that the average duration of fixations of the eyes in reading, even for adults, is approximately one-fourth of a second. If the easily attainable tachistoscopic rate of a one-hundredth-second exposure could be maintained in reading, simple multiplication would give a speed of 6,000 words per minute. No one has ever

* From an address before Sections I and Q, A.A.A.S., Boston, December 28, 1946.

proposed that this is attainable, nor am I implying that psychologists employing tachistoscopic methods hold any such beliefs. However, in view of the statement, supported by evidence, that tachistoscopic training results in improvement in reading, there is some necessity for understanding the nature and the range of such improvement.

Obviously, perceptual data from the tachistoscope and the eye-movement camera are of two kinds. The tachistoscope measures the time required for a single recognition. It does not follow that another word could be exposed and recognized in the next one-hundredth second, and still another in the following one. Recognizing *a* word in one-hundredth of a second does not mean that *words* can be recognized at a rate of 100 words *per* second. An experimental attempt to do this with a kinetoscope projector would at once illustrate the difficulty. Reading involves the continuous recognition of consecutive verbal symbols with the subsequent fusion into units of meaning. It must be, therefore, that improvement resulting from tachistoscopic training is to be explained in the changed character of the perceptual experience, that is, centrally rather than peripherally. Renshaw recognizes this point of view in his emphasis on "field structuring." He summarizes his position as follows:

> In tachistoscopic work the exposure time/length of material relation is a function of grouping and field-organization in perception. Concepts such as the "span of attention" or "span of visual apprehension" must be specified in terms of the developmental level of skill of the perceiver. The limits of improvement from training by tachistoscopic methods are undetermined. The control of motivation and the active restructuring of the visual field by the perceiver seem to set the limits of improvement through training (*J. Psych.*, 1945, **20,** 230).

The essential characteristic of the tachistoscopic method is that it provides a means of controlling a single perceptual experience; that is, it so limits the time of exposure that not more than one fixation of the eye is possible. Precision tachistoscopes are superior to flash cards for this reason. As flash cards are frequently used, the duration of exposure is long enough for two or more eye fixations. When movements of the eye are thus possible, the total perceptual experience may be a series of impressions, in which case what is seen on the second and later fixations may be added to what is seen on the first. This is a crucial distinction. It is a summation of stimuli or elements as contrasted with a single "structured" or integrated pattern that is observed. The distinction is well illustrated by the methods of aircraft identification used during the war. The original WEFT system (wings, engine, fusilage, tail) represented a series of rapid impressions of each part. The later practice in identification was based on the assumption that a single impression would give the plane "as a whole." The tachistoscopic practice of flashing the image of an airplane on a screen was carried out at a rate of exposure which guaranteed that perception actually was limited to a single fixation of the eyes.

This article is not concerned with military applications of perceptual research but rather with its use in learning in school and particularly in the case of learning to read or to improve one's ability to read. Here the situation, operationally, is quite different from aircraft identification. The process of reading involves a continuous series of perceptual experiences, and the scope of each perceptual unit (the words or letters perceived in a single eye fixation) varies according to the limitations of the central process of recognition and interpretation. The chief values to be gained from tachistoscopic exercises would be in (*a*) reducing the duration of the fixation pauses or (*b*) widening the span of recognition; that is, increasing the number of words recognized in a single eye fixation. It is unfortunate that the two principal experiments in im-

proving reading by tachistoscopic training, namely, Renshaw's experiment in Gary, Ind., and MacLatchy's experiment in Bexley, Ohio, did not include records of eye movements in their data. Such records would have furnished objective and precise measures of the nature and amount of perceptual changes that accompanied the reported improvement in reading.

Two principal factors have been reported that influence perception in reading, and these have constituted the major basis for changes in methodology. One of these has to do with the perceptual organization of the material being read, and the other with the degree of freedom from vocalization in the reading process.

The history of methods of teaching reading shows a striking parallel between prevailing psychological beliefs regarding the nature of perception and the practices used in the schools. The *New England Primer*, widely used in early American schools, was constructed in harmony with the view that perception was a summation process. This book began by teaching the letters of the alphabet, both small letters and capitals. This was followed immediately by teaching combinations of two and three letters. In some cases these letters made words, but in others only meaningless combinations. Next, words were introduced, first those of one syllable and then in succession those with two, three, four, and five syllables. Continuous reading material was introduced following the five-syllable words.

The theory supporting the *New England Primer* was that letters were the units of perception and that the normal sequence was from letters to syllables, then from small to larger words, and finally to continuous reading material. Syllables were the sum of their letters; words the sum of their syllables; sentences the sum of their words. The findings of both tachistoscopic and eye-movement research have demonstrated the falsity of this concept. It has been shown that even large words can be recognized as wholes and that with improved methods of teaching the span of recognition in silent reading can be two or more words. In carrying on the process the reader may not even be aware of words as such but only of their fusion into a continuous fabric of meaning.

An interesting example of an early awareness of a superior method of reading is afforded in Horace Mann's *Second Annual Report* as Secretary of the Board of Education of Massachusetts in 1838. Mann wrote as follows:

> When a motive to learn exists, the first practical question respects the order in which letters and words are to be taught; i.e., whether letters, taken separately, as in the alphabet, shall be taught before words, or whether monosyllabic and familiar words shall be taught before letters. In those who learnt, and have since taught, in the former mode, and have never heard of any other, this suggestion may excite surprise. The mode of teaching words first, however, is not mere theory; nor is it new. It has now been practised for some time in the primary schools in the city of Boston,—in which there are four or five thousand children,—and it is found to succeed better than the old mode.

A later and highly important example of the effects of perceptual research on methods of learning is afforded by the change from oral to silent reading practices in elementary schools. Although this silent reading reform did not reach its climax in American schools until the decade from 1915 to 1925, it was foreshadowed in the report of Horace Mann above-mentioned. On this issue Mann wrote:

> I have devoted especial pains to learn, with some degree of numerical accuracy, how far the reading, in our schools, is an exercise of the mind in thinking and feeling, and how far it is a barren action of the organs of speech upon the atmosphere. My information is derived, principally, from the written statements of the school committees of the respective towns,—gentlemen who are certainly exempt from all temptation to disparage the schools they superintend. The result is, that more than eleven-twelfths of all the children in the reading-classes, in our schools, do

not understand the meaning of the words they read; that they do not master the sense of the reading-lessons, and that the ideas and feelings intended by the author to be conveyed to, and excited in, the reader's mind, still rest in the author's intention, never having yet reached the place of their destination.

It is easy to demonstrate that the perception of words may be followed by a series of oral responses without any attention to the meaning of what is read. Even today schools may still be found that cling to a method of oral reading that is frequently nothing more than a calling of words. However, such practices are now the exception rather than the rule. The scientific basis for this change in methodology is found in the early studies of eye movements by Judd, Dearborn, and C. T. Gray. These studies showed that the prevailing pattern of perception in oral reading consists of one or more fixations for each word, while for silent reading the span of recognition was generally a group of two or more words. The superiority of the silent-reading process on the basis of perceptual factors was unassailable. The retarding effect of vocalization was clear. It is difficult to find examples of research in psychology that have had as widespread effects on methods of learning as have the studies of visual perception in oral and silent reading. The evidence was so impressive that, at the present time, the major teaching of reading in the elementary school employs far more silent reading than was the case during the earlier period, and in most elementary schools the proportion of reading experience now is much greater for silent reading than for oral reading. While this silent-reading reform appears on the surface to be far-reaching in effect, a closer analysis of the outcomes of the movement indicates that the change was more superficial than many of its advocates presume. It was superficial in the sense that silent reading may be carried on, and in many cases is carried on, by exactly the same psychological process as oral reading, except for the fact that there is no oral vocalization of the words. There is still for many readers subvocalization, or, as it is commonly called, "inner speech," accompanying reading.

Although, in principle, the schools have quite generally accepted the idea that the basic process in reading is a direct association between the words as perceived and their fused meaning, there are still many schools that have not sensed the full import of this relationship. There is still a considerable degree of intermixture of oral and silent reading in the primary grades. As a result, what often passes as silent reading is only noiseless reading. Psychologically, the oral process still persists, only instead of pronouncing the words aloud the child whispers them to himself, as evidenced by lip movements. The process of subvocalization may be so far suppressed that no lip movements or throat movements are observable, but the reader still is conscious of reading word by word instead of being conscious of the meaning only. Completely silent reading, or "nonoral" reading, as it is sometimes called, is carried on without subvocalization or consciousness of words as words. It is similar to the auditory perception of speech. Few persons listening to a lecture follow the speaker by subvocalizing after him the words he speaks. If anyone doubts this, let him try to subvocalize after a speaker for a moment or so; he will soon be aware that auditory perception generally carries on by direct association between stimuli received by the ear and recognition of meaning without the accompaniment of subvocalization. Reading, at the most effective level, is a similar process.

In 1935 the public schools of Chicago developed a method of teaching such nonoral reading for use in Grades I and II. In the ensuing ten years more than 70,000 pupils in 137 different elementary schools in that city learned to read by this method. In 1945 I made a survey of the results of this

method. Four hundred sixty-five pairs of pupils at the sixth-grade level were studied, one member of each pair having been taught in Grades I and II by the nonoral method, and the other by the conventional mixture of oral and silent reading. The pairs were closely matched by age, school grade, and I.Q. Pupils were compared on the basis of scores on standardized reading tests, absence of lip movement, and acceleration or retardation. Comparisons were also made for bright and dull children separately. The published results of this investigation indicated a superiority for the nonoral method in every one of the eight criteria used in the study. The nonoral method did not eliminate lip movement in all cases, as by theory it should have done, but its superiority over the customary method was unquestionable.

These changes in methodology should now be related to the studies of perception with which this discussion began. The situation may be summed up as follows: The many published studies of oral and silent reading agree that in the process of silent reading the span of recognition, as indicated by the average number of words per fixation, is markedly wider than in the case of oral reading. Likewise, the duration of fixations is notably shorter. The rate of oral reading averages approximately 175 words per minute at the adult level, whereas the rate of silent reading for similarly selected persons and materials will average approximately 300 words per minute. The difference in rate is due primarily to the slowing effect of the articulation process. Persons who subvocalize in silent reading have a much slower rate than those who suppress all tendencies to deal with words separately. Where subvocalization is suppressed, rate of reading is a function of rate of comprehension and rate of perception. Since the only purpose of reading is to comprehend, rate should be determined primarily by ability to comprehend. Rate of comprehension should not be restricted by inadequate perceptual processes. Improvement in the perceptual aspects of reading is still the best chance to contribute to efficiency in reading. This cannot be realized without freedom from the restrictions of subvocalization.

The position stated in the preceding paragraph is not the result of so-called armchair reflection. As director of the adult reading clinic of The University of Chicago, for the past five years I have listened week after week to otherwise competent adults complain that their rate of reading was so slow that they were unable to keep pace with the amount of reading they should do. Half of these adults were graduate and professional students. In a striking number of cases tests showed that their rate of silent reading was very close to their rate of oral reading. Eye-movement records for these persons showed for their silent reading all the characteristics of oral reading—fixations on practically every word and longer than average duration of fixations. The fact was that they were subvocalizers—the victims of a method of teaching reading that fixed oral-reading habits first and so strongly that the later silent reading was only noiseless reading, showing none of the characteristics common to effective silent reading. In the reading clinic, using techniques that gradually forced a rate of silent reading beyond that at which articulating is possible, most, but not all, of them succeeded in setting up and fixing a perceptual pattern commensurate with a silent reading process. In the course of twenty one-hour corrective exercises, 234 subjects made an average gain of 54 percent in rate without loss of comprehension. This gain in rate was accompanied by an increase of 29 percent in the number of words per fixation and an increase of 13 percent in speed of fixation time. These changes were primarily perceptual. For most of the cases comprehension was satisfactory at the start. Their problem was rate of reading, and the difficulties were in perception rather than in comprehending what was read.

The customary average rate of reading nonfiction at the end of the program of instruction in the schools is 300 words per minute. Rates running down as low as 150 words per minute are not uncommon both in high school and in college, and the variation is roughly in proportion to the amount of subvocalization present. Above the average of 300 words per minute, cases will be found which, in reading nonfiction with comprehension (not scanning), reach rates of 600 to 800 words per minute. As has been stated, comprehension should be the determiner of rate. In fields where one is familiar, and with due consideration for flexibility in terms of purpose and content, there is little reason for continuing to accept as satisfactory a rate of reading that is a product of a considerable degree of subvocalization and of perceptual habits that are much inferior to those known to be attainable. If schools can be persuaded to accept the fact that, in essence, reading is a process of direct association between perceptual stimulus and meaning without any intervening subvocalization, and, if they can get the full import of the statement that reading is a process of thinking the meaning rather than saying the words, students in perceptual research can explore the limits of efficiency in perception and their relation to the controlling factor of comprehension.

AT THE AIRPORT

Innumerable swirling saws, cutting at the air,
The great level cement walks, rimmed by lights—
I walk under the dream of all these planned spaces,
And remember Leonardo da Vinci, living in the streaming wind.
I see the brooding eyes of the old man, the orbs peering at centuries;
And these wings that a man dreamed are monuments to him.
At the airport, the motors take over—and the winds of the future.

DANIEL SMYTHE

Book Reviews

PREHISTORY

Dating the Past. An Introduction to Geochronology. Frederick E. Zeuner. xviii +444 pp. Illus. 30*s*. Methuen. London. 1946.

THIS book by the distinguished Professor of Geochronology and Environmental Archeology in the University of London is a pioneer in a relatively young field. It covers the whole field of geochronology but emphasizes especially the chronology and climates of the Pleistocene (Ice Age), on account of their practical importance in prehistoric archeology and human paleontology.

Part I deals with dendrochronology, or tree-ring analysis, developed by Douglass, Huntington, Antevs, and others, and its use in dating early history and late prehistory of man.

Part II describes varve analysis, the varve chronology, pollen analysis, and the correlations of the time scales and climatic records shown by varves and pollen profiles with the record of raised beaches. The late glacial and postglacial chronology so obtained is then applied to the important prehistoric sites of the late Old Stone Age, Middle and New Stone Ages, and the Metal Ages. It carries the dating back to about 15,000 years ago.

Part III is devoted to the chronology of the Ice Age and Paleolithic man. Using radiation curves calculated by Milankovitch, the author develops an absolute chronology as follows: Duration of the entire Pleistocene, 600,000 years; double climax of early glaciation, 590,000 and 550,000 years ago; of the second ("Antepenultimate") glaciation, also double, 476,000 and 435,000 years ago; of the third ("Penultimate") glaciation, likewise double, 230,000 and 187,000 years ago; and the triple climax of the last glaciation, 115,000, 72,000, and 25,000 years ago. The interglacial intervals are found to be 60,000, 190,000, and 60,000 years long, respectively.

This chronology is then applied to the various Paleolithic sites of central and eastern Europe, Siberia, northern France, Portugal, the Channel Islands, and the British Isles.

The Pleistocene pluvial climates of the Mediterranean region are correlated with the glacial stages elsewhere, and tentative dates are assigned to Paleolithic sites in that area also. The author then reviews the dating of past climates, early man, and industries in Africa, Asia, Australia, and America.

Finally, a table of approximate dates is given for each of the several culture or industry stages of prehistoric man and for the fossils of man himself. This chronology of early man and his cultures forms the core of the book. As it falls within the main interest and competence of the author, it represents his main contribution.

Part IV deals with a variety of topics—the measurement of geological time, the age of the earth and the universe, the time-rate of geological processes, the time factor in biological evolution, the time-rate of development of species, and related chronological subjects. These extend the geochronology to about 2,000 million years.

The work is concluded with a 33-page bibliography (arranged by chapters), a 22-page index, and a section of plates.

The book, replete with chronological tables, both relative and absolute, will appeal mainly to workers in archeology, anthropology, and Pleistocene stratigraphy, and less directly to those in climatology, geography, geology, paleontology, and biology.

IRA S. ALLISON

Department of Geology
Oregon State College

DIFFICULT PROBLEM

Drought, Its Causes and Effects. Ivan Ray Tannehill. xii + 264 pp. $3.00. Princeton Univ. Press. Princeton. 1947.

IN *Drought, Its Causes and Effects* the author, who is Chief of the U. S. Weather Bureau's Division of Synoptic Reports and Forecasts, has skillfully handled an exceedingly complex subject in a manner that will appeal to both laymen and scientists. The reader is made to feel that he is standing beside the author, so to speak, when weather records, sunspots, tree rings, maps, and charts are being examined for clues to the drought problem.

In the closing paragraph of his introductory note the author says:

> In this book we shall proceed with an open mind. First we shall look at the nature of the problem and see what drought, with its associated crop failures, famines and dust storms, means to the people of the world; and we shall review briefly the history of droughts in the United States. Next we shall look at the facts in the case from the standpoint of weather. We shall review the rainfall records and related weather data and look for a clue. Third, we shall try to put together the pieces of this great puzzle and see if we have a satisfactory explanation and any hope of predicting drought in the future.

Between this introductory note and the conclusion we find several chapters of delightful and informative reading. The chapter on Famine, showing how several nations on earth are frequently visited by starvation because of insufficient rainfall, emphasizes the need for continuous study about drought and its cause. Readers will probably vary in their choice of the intervening chapters. Some will be impressed with the discussion of sunspots and their relation to weather, and others will be more interested in the chapter dealing with the Pacific as a weather barometer.

The twentieth and last chapter is a summary, and the author says in part:

> In conclusion, the writer regrets that it is necessary to say that the problem of drought is not completely solved. A great deal of work remains to be done. The changes of the future may bring new phases of the problem which will have to be studied.
>
> The pattern of world rainfall is vital to human civilization. Time has scrawled a bold but cryptic message, alternately dry and moist, on the rocks, on the fossils of flora and fauna of the ages, and finally on the gravestones of civilizations. The scrawl continues, and at last our thermometers, rain gages, barometers, rawinsondes, and other instruments are beginning to decipher the story. The record shows unmistakable evidence of the interactions of atmosphere, oceans, and continents, under the basic control of the sun.
>
> In the future, farmers will not have to gaze despairingly into a clear sky, wondering if a few clear days will continue into a disastrous drought. Even if we are never able to control the climate, much will be gained by knowing what to expect. Droughts are not mere chance occurrences; they are part of a physical process which can be measured and studied and predicted with increasing precision as our observations of the sun and the upper air and the oceans continue to accumulate.

From a critical point of view it seems to me that hopes are aroused in the earlier chapters that are not fulfilled in the book. Then, too, I sensed the implication that farmers cannot do much about droughts, that they must suffer when it comes or call for governmental relief until the rains return. We know that during the drought period of the thirties many farmers in the worst of the old dust bowl area were able to keep cover on their land. They didn't

make much money, to be sure, but they kept their land intact because their cropping system was adapted to the hazards of the climate. We learned also that some farmers, through skillful management of the water supply, grew crops when their less provident neighbors failed on adjoining lands.

What the author missed is the fact that under effective conservation measures, particularly contouring, much or all of the rainfall reaching the land is stored in the reservoir of the soil. This not only aids with conservation of the soil by increasing the growth of soil-holding vegetation, but it is the equivalent of changing the climate insofar as the life of plants is concerned, especially in the Great Plains.

To date conservation measures have been applied to something over ten million acres of land in the former dust bowl. If a series of dry years returns, the Great Plains are in far better condition to withstand the damaging effects of drought than they were in the thirties.

GLEN K. RULE

Soil Conservation Service
U.S.D.A., Washington, D. C.

MONSTROUS DRAMA

The Last Trek of the Indians. Grant Foreman. 382 pp. Maps. $4.00. University of Chicago Press. Chicago. 1946.

FOR the scientist layman living in the East, Middle West, or Oklahoma who wants to learn what happened to the Indians of his state and how Oklahoma came to harbor remnants of some fifty-five tribes, of whom but six were indigenous, this books holds the answer. It completes a study of Indian removal, which Grant Foreman began by describing how the southeastern Indians were displaced, and accounts for what happened to the broken fragments of tribes who were living north of the Ohio River when James Monroe became President in 1817.

Except for the New York Iroquois, who wisely resisted resettlement, the typical northern tribes were the Delaware and Shawnee, who were so used to being pushed around that they retained only a semblance of old tribal organization and a tradition of an eastern homeland. They were the victims of intertribal and colonial wars and the Revolution; they had abandoned gardens and village sites, often twice in a generation, and removed, generally westward, before the advancing white man "... to find better hunting or maple sugar groves or huckleberry country; to flee from epidemics; or to follow the fortunes of a rival chief or medicine man...." They broke into factions and scattered in many bands to be thrown with strange bedfellows like the Wyandots and Senecas of Sandusky, both of Iroquoian stock.

At the turn of the eighteenth century the young government undertook to remove the Indian residents of Ohio, Indiana, Illinois, and New York—Delaware, Shawnee, Ottawa, Wyandot, Kickapoo, Potawatomi, Peoria, Kaskaskia, Piankashaw, Sauk and Fox, Cayuga, and Seneca—without plan or consistent policy, predetermined method, or personnel familiar with the history and ethnology of the peoples involved. Ethnology was not yet even in its infancy, and a century and a half were to pass before its findings were applied. Instead, the pressure of political expediency ruled the day, the corruptibility and cupidity of Indian chiefs were played upon through bribes and the persuasive properties of whisky, and with flagrant disregard of human rights the agents of land companies stooped to murder and arson to secure land cessions for a hungry frontier. The documents that Foreman has assembled to support his brief provide a sad commentary on the beginnings of "free enterprise" in the Middle West.

Yet the picture is not clear-cut since history has hopelessly complicated the situation for the scholar; Foreman has been prevented by the sheer mass of evidence and its diversity from clarifying the material for the reader.

Reports of His Majesty's Indian superintendents in America, the similar manuscript reports of the Department of Indian Affairs (at first a branch of the War Department, afterward of Interior), and the correspondence of Army officers, Indian agents, missionaries, and schoolmen constitute the literature on Indian removal and now repose in the National Archives and in the collections of various libraries and historical societies. Besides these sources, Foreman has used the printed reports of the Commissioner of Indian Affairs (1824–1907), Congressional documents, bills, and hearings, the U.S. Statutes at Large, and the American State Papers. But, aside from the *Handbook of American Indians*, ethnological reports on the tribes removed were overlooked.

The fore part of the book is concerned with removal; the latter and major part with what happened when the Indian tribes reached Kansas, only to be uprooted and resettled in Indian Territory, now Oklahoma. The reviewer has followed each group on its long trek, and he has no reason to question essential facts or treatment of sources. It is a good job of history, but there are so many treaties, negotiations, and removals that each new group treated involves repetition that grows wearisome despite the intrinsic interest of the material and, like the atrocity tales, dulls the sensitivities. No clear design emerges in the treatment, and a more general summary would have helped the average reader. As it stands it is a scholar's book and a fine contribution.

WILLIAM N. FENTON
Bureau of American Ethnology
Smithsonian Institution

HISTORY OF ANESTHESIA

Victory over Pain. Victor Robinson. xiv + 338 pp. $3.50. Henry Schuman. New York. 1946.

THIS is a scholarly account of man's search for suitable anesthetic agents. Its publication coincides with the one hundredth anniversary of the first successful public demonstration of ether anesthesia. We tend to forget the magnitude of the miracle of modern anesthesia and the momentous changes in surgery and obstetrics that it has permitted. Even the anesthetic properties of so simple a substance as ice were not recognized until the past decade. How many agonized screams have echoed through the years because surgeons did not know of this wonderful power of frozen water! It is interesting to pause occasionally to take stock of what we have done and perhaps to wonder why scientific progress has been so painfully slow in many great periods of the past.

Each of the many types of anesthetics available today has a turbulent history associated with its development. The inevitable clash of personalities seeking personal glory and the social and religious taboos which defy change are colorfully portrayed by Professor Robinson in this absorbing volume.

Before the advent of modern anesthesia, surgical procedures were rarely employed. They were as much dreaded by the surgeon as by the patient. Surgery was a desperate measure employed only when the alternative appeared to be the death of the patient. The skill of a surgeon was measured in minutes rather than by his ability to perform the operation properly, and the patient always arranged his affairs beforehand as if his last hour were approaching.

The science of anesthesia as we know it today is the product of work done principally in the last century. More progress

has been made during the past one hundred years than in all prior history. Each period discussed in the book is made vivid with anecdotes and extracts from pertinent letters of the time which are of interest to the layman as well as the physician. The story starts with the early Greeks, who were never successful in doing more than dulling the sensibilities minimally with plant products, and the Chinese, who undoubtedly made contributions which have become lost. The arrangement is chronological, and each chapter deals with a new period.

In times such as these it is refreshing to contemplate a science which was crystalized from a mixture of investigative genius, personal feuds, jealousy, ignorance, and prejudice to become one of the major contributions that man has made to his own welfare.

RICHARD B. BERLIN

Naval Medical School
Bethesda, Md.

NO MORE FRONTIERS

Radio's Conquest of Space. Donald McNicol. x+374 pp. Illus. $4.00. Murray Hill Books. New York. 1946.

DRAWING from a lifetime of intimate association with the pioneers of the wireless industry, Donald McNicol has in this book presented the history of radio achievement in a manner most readers will enjoy. The book is nonmathematical, and it will be found easily readable.

Mr. McNicol explains clearly and concisely for beginners the operation of each device. This detailed explanation regarding the functions and particular advantages of new apparatus over its predecessors enables the reader to appreciate better the contributions of each inventor. The author goes to unusual lengths to maintain historical accuracy, even at the expense of repetition. Of particular interest to the younger generation of radio men are the youthful endeavors of men who later became heads of large manufacturing firms bearing their names.

To those who have spent many years in the radio profession this book will have a strong appeal because of the down-to-earth presentation of the struggles of men of genius, without any attempt to dramatize their achievements. The veteran engineer will be delighted as McNicol recalls the operational procedures and the equipment used in the horse-and-buggy days of wireless. Newspaper notices, personal correspondence, and extracts from professional journals are of special interest to men who will remember having read these articles when first published.

In arranging topics for discussion, the author has eliminated confusion by devoting each chapter to the chronological development of a single device. Other qualities which are unusual and commendable are the little asides which the author makes regarding politics, commercial programs, and mythology. Philosophical observations, the inalienable right of all mature authors, add spice to this interesting narrative. The competence of the author to handle his subject is eloquently revealed by the intimate personal anecdotes which he relates about his friends who were the makers of radio history. At times, however, McNicol assumes that his readers will all have the same familiarity with the lives of the radio great that he enjoys; so that in some cases the curiosity of the reader is only whetted and not satisfied. Possibly this is a virtue and not a fault.

The physical make-up of the book is average. The latter part of the book is well illustrated, but there are not enough pictorial or schematic diagrams to illustrate unfamiliar obsolete equipment.

HAROLD D. CALLAHAN

Capitol Radio Engineering Institute
Washington, D. C.

ROGUE RIVER

The Colorado. Frank Waters. xvi + 400 pp. Illus. $3.00. Rinehart. New York and Toronto. 1946.

Listen, Bright Angel. Edwin Corle. viii + 312 pp. $3.75. Duell, Sloan and Pearce. New York. 1946.

ABOUT any river there is an inexhaustibility that defies all learning. But what are we to do with such a river as the Colorado and its Grand Canyon, so mighty in power and splendor as to shrivel the imagination? A thousand books could be written about this river, and every one might be a good book, yet every one would fail to do full justice to it. For the Colorado is an eternal paradox. It is a place where angels and devils meet. It is the most revealing natural phenomenon in America, yet the most incomprehensible.

Frank Waters and Edwin Corle are two Southwesterners who independently and almost simultaneously decided they knew the Colorado River well enough to write about it. Although they produced somewhat similar books, which occasionally overlap, they are different enough to make it profitable to read both. They are not, of course, strictly "scientific" books, for the authors are not scientists or technologists—they are writers*—but if the science in them were taken out, not much would be left. Some of the sciences that the reader will have the pleasure of meeting, though superficially, are:

Geology. The whole Colorado River country is a geologist's paradise. All thinking about the Grand Canyon must begin with the question: How long has this been going on? Geologists are still studying the age of the river, with perennial answers to the question. (See Professor Chester R. Longwell's article, "How Old Is the Colorado River?" in the December 1946 issue, *American Journal of Science*, for the latest summary of the problem.) Like the Colorado itself, geology runs deep through both of these books, clarifying our understanding and enhancing our appreciation.

Anthropology. The people of the Colorado country are inseparable parts of both books, but see especially Corle's Section V, titled Land of the Sky Blue Water, which includes a fascinating, humorous, and firsthand account of the Havasupai Indians, who have lived since God knows when in their labyrinthian canyon home along Havasu Creek, now within Grand Canyon National Park, Ariz. In this country, too, flourished the ancient cliff dwellers and the great pueblo peoples, and the story of the Colorado is their story also.

Engineering. Man naively thinks that he has conquered the Colorado with Hoover Dam, in the colossal building of which 110 men lost their lives but which reclaims 2,000,000 acres of desert and illumines and empowers the city of Los Angeles and her numerous satellites. But wait a few years and see, say Messrs. Corle and Waters. It may be that the river will have the last say, thinks Waters. Says he:

> Capable of impounding the river for two years, Boulder Dam also retards its movement of silt. The water, settled and clear, is released; the silt remains. Within 50 years Lake Mead will be filled with water; in 300 years the whole vast reservoir will be filled in solid with sand and silt.

* In biography Waters has written *Midas of the Rockies*, the story of Winfield Scott Stratton and the history of the Colorado gold discoveries; he is also the author of, among other books, two historical-ethnological novels, *The People of the Valley* and *The Man Who Killed the Deer*, the latter descriptive of Pueblo life and ceremonialism. Corle also is a novelist and has written two of the best American Indian stories I have ever read: *Fig Tree John* (Apache) and *People on the Earth* (Navaho). Among his other works is a regional book on the Southwest, *Desert Country*. All of them deserve a wide reading.

> Another dam, probably in Bridge Canyon, must be built farther upriver. Then still another until all possible 11 sites will have been utilized and exhausted. Then again the Colorado will resume its way. Which one of us dares assume that one transient race of men . . . can do more than retard for a geologic moment the river's immemorial and immeasurable task of transporting bodily the whole vast Colorado Pyramid into the sea?

A nice question, and Waters takes the long view and hopes he won't be anywhere around when the dam goes out.

Navigation. The attempts to navigate the Colorado, from either the north or the south, have been shining examples of man's egotism. There have been a few successes and many failures. These books cover this subject to some extent, especially Lieutenant Joseph C. Ives's famous *Explorer* expedition of 1858, but one may go to another recent book, Jerry MacMullen's *Paddle-wheel Days in California* (1944), for a fuller account of Colorado River navigation.

Agriculture and irrigation. Waters' story of the opening up of the Imperial Valley to agriculture, involving some pretty tricky and costly plumbing paid for by private individuals, corporations, and government, forms an exciting chapter.

Geography and exploration. The explorations of Major John Wesley Powell, naturalist, geologist, ethnologist, and the first man to travel through the Grand Canyon of the Colorado and live to tell the tale, are features of both books, especially Corle's. Powell was an important as well as a colorful figure in American science and deserves a full-length biography by a competent historian. Later explorers of the river —scientists and adventurers alike— all figure in these chronicles.

Biology. This subject is scantily treated, but Corle summarizes the wildlife and forest resources, especially of the Kaibab country on the north rim of the Canyon.

One could go on. There are portions of both books, too, that are purely historical in nature, retelling the story of the early Spanish travelers—conquerors and padres—who first found the Colorado country and massacred or missionized the Indians, leaving their ineradicable marks. One such mark is the name of the river itself—Río Colorado. In both volumes also there is a personal quality that makes them pleasant to read; for both authors have so thoroughly identified themselves with the Southwest that their own experiences become a part of the chronicle and give a lively, authentic, and individual ring to what might otherwise be a dull documentary discourse. *The Colorado* is the twenty-ninth in the now well-known Rivers of America series. The book is illustrated with drawings by the Russian-born artist Nicolai Fechin, and Waters sets great store by these pictures; unfortunately, however, they are not reproduced well enough to render their full values, being printed from coarse-screen half tones on regular text paper. Included also is one rather muddy oil sketch of an Indian ceremonial reproduced in color. *Listen, Bright Angel* is unillustrated except for a map or two.

PAUL H. OEHSER

Smithsonian Institution

BIRD NOTE

Audubon Bird Guide: Eastern Land Birds. Richard H. Pough. xxxvi+312 pp. Illus. $3.00. Doubleday. Garden City, N. Y. 1946.

IN THIS guide the 275 species of land birds that occur in the area of eastern North America are each described—males, females, and sometimes juveniles. Concise paragraphs treat the identification, habits, nests, voices, and distribution of each bird.

The author discusses briefly and generally psychology and behavior, ecology, migration, and economic relations. There are observations on conservation, the effects of

insecticides, and even information on the selection and use of binoculars for bird studies.

The detailed figure of the topography of a bird will be helpful to amateurs. The 48 excellent color plates have over 400 illustrations that show not only the male and female plumage and sometimes the juvenile, but also seasonal variations, and are of great value in identifying the species.

There is a bibliography and a most excellent index, and the book is bound in a flexible cover. Out of five of the reviewer's friends who glanced at the book as it lay on his desk, four used the word "concise," and two went out to buy copies.

WILLIAM M. MANN
National Zoological Park
Washington, D.C.

HANDBOOK

Household Physics. Madalyn Avery. Rev. ed. xi+470 pp. Illus. $4.50. Macmillan. New York. 1946.

THE primary function of any elementary textbook is to set forth basic principles. When the text is an "applied physics" written for use in a college and for a particular group of students, the examples and applications should be chosen from the field of the students' experiences or so as to give information of use when the student at some later time encounters any of the phenomena involved, either in a course or in some life experience. This principle, in general, is well adhered to throughout this text.

In addition to the presentation of material from which household applications can be drawn, most of the rest of the entire subject matter of an elementary college physics text has been included, the revision rearranging the subject matter into the divisions and order most often found in a college text. Apparently because power electrification is reaching rapidly and extensively into rural areas, the labored discussion of individual farm electric plants has been eliminated. The extensive discussion of the sewing machine, a favorite piece of equipment upon which to base a household physics text, happily has been deleted.

Mention is made of an apparatus for determining metabolic rate and bodily energy expenditure. An airtight mask fitting over mouth and nose is a constituent part of the apparatus, and yet it is indicated as useful in determining the energy required in eating a meal—an obvious impossibility.

It seems evident the text is written to serve two purposes: to serve as a basis for a physics course, and to give instruction in household equipment. Appearing in various places in the book are directions as to what to look for in selecting equipment, how it should be constructed, and instructions as to how to take care of it. Many of these items are outside the pale of a physics course.

Although the revision is recent, the material on household equipment is not up to date. The better ice refrigerators are not constructed as shown in the illustrations. Electric range units, about which much discussion is centered, do not include the more recent ones with "infinitely-variable" heat. Only a brief discussion of the home freezer is given when the present apparent interest in the subject seems to merit a more extensive treatment.

It is in the fulfillment of the second purpose that the text is incomplete. It might be argued that the instructor should be able to elaborate upon the principles and applications involved, thereby filling in the gaps caused by omission or contraction; but there are not enough physics instructors who keep abreast of developments in the field of household equipment to make such a makeshift course substitution a success.

EARL C MCCRACKEN
Bureau of Human Nutrition
and Home Economics
U.S.D.A., Beltsville, Md.

Comments and Criticisms

POLLEN COUNTS

Would it be out of order to call attention to a technical error in one of the honorable mention articles under "The AAAS-George Westinghouse Science Writing Awards for 1946?" (SM, January 1947)? I quote from the third paragraph on page 81: "After exposure the slide is treated with a red dye which is absorbed only by the ragweed pollen." The author evidently refers to Calberla's solution with fuchsin which is often used for staining atmospheric pollen samples. While this stain penetrates the exine of pollen grains more readily than most other plant tissues, it is *not selective for ragweed pollen.* The author probably intended to bring out the point that stained pollen grains are readily differentiated from other small particles on the slide such as sand and soot.

One could also find considerable fault with the second and third sentences of this same paragraph where the author slips so lightly over the matter of counting the pollen on a gravity slide and interpolating the results in volumetric terms.

O. C. DURHAM

Abbott Laboratories
North Chicago, Ill.

ESKIMO INFANTICIDE AND POLYANDRY

There is a story of the man who declared that all Indians walk single file. When asked what evidence he had for this, he replied, "The one I saw did."

Professor Garber in his interesting article on "Eskimo Infanticide" (February 1947 SM) derives Eskimo polyandry from a shortage of women caused by female infanticide. He cites an instance in which the St. Lawrence Island natives sent him word that they needed a large number of marriageable girls. This is not evidence of an actual demographic unbalance of the sex ratio. Polygyny is so common among Eskimos that some men are commonly left without wives. Violent sexual competition among the males is so strong that it is quite possible that an Eskimo group would be only too happy to have the government supply them with more women if that could be done.

Population statistics indicate that Professor Garber is quite wrong in his assertion that female infanticide results in a shortage of women. On the contrary, occupational hazards for males are so great that in most Eskimo communities there is a surplus of women over men in spite of the fact that a large percentage of the girl infants are killed.

In 1924 Edward Weyer, in *The Eskimos*, (Yale Univ. Press, 134–135), brought together data from a number of Eskimo groups outside East and South Greenland (where infanticide is rare). The ratio of girls to boys under ten years of age ranged from forty-two to ninety-two girls to each one hundred boys. Among adults over fifteen years of age the proportions were reversed. Out of twenty groups on which data were available only three had fewer women than men. In more than half of these groups there were more than one hundred and ten women to one hundred men.

These wider facts indicate that Eskimo polyandry cannot be due to a shortage of women as caused by infanticide. The shortage is caused by the sociological fact of widespread polygyny. Strong men monopolize the available women and weak men are left out. This, combined with a strong principle of the sexual equivalence of brothers, results in fraternal polyandry.

Anthropologists have learned to be extremely wary of simple biological explanations of cultural phenomena. While not rejecting the possibilities of biological influences on human behavior, we find that it is not safe to overlook cultural factors when explaining cultural phenomena.

E. ADAMSON HOEBEL

New York University

APPRECIATION

I'd like to comment on what I think has been a definite improvement in the character of the articles included in the last dozen or so issues of the MONTHLY. In the current number "On Life as a Separate Entity," by Thomson King, is especially fine, and I have taken the liberty of writing directly to Mr. King complimenting him on it. I think the MONTHLY is serving a very definite purpose for those of us who are engaged in working with the industrial applications of science. To open and read it each time is like opening and passing through a broad, high gate yielding upon ever-widening vistas of knowledge. To me it is an indispensable experience.

CHARLES F. JOHNSON, JR.

Watertown, Conn.

The Brownstone Tower

As a layman in the social sciences, I should like to discuss the current situation in that field as it looks to me from my bay-window substitute for the Brownstone Tower. Social scientists are in an uncomfortable position. In the present precarious state of the world they know that their subjects should be more important for the future of the human race than any of the natural sciences. But they cannot yet convince natural scientists, or the public, or even themselves that they *are* scientists and can use the scientific method on human problems in an unbiased and potentially fruitful manner. They seem to be frustrated in their public relations, and their offerings to the SM are characteristically argumentative, insisting, with theoretical reasonableness, that since man is a part of nature, students of human society should be regarded as natural scientists. There is no question that they have performed in the manner of natural scientists in their studies of cultures of the past and those of contemporary backward peoples, but it remains to be proved, at least in the SM, that they have demonstrated their capacity to act as natural scientists in studies of current problems of so-called civilized societies.

While social scientists are trying desperately to secure public recognition as natural scientists and to gain public confidence, they are continually being embarrassed by encroachments into their own fields by natural scientists, clergymen, philosophers, politicians, and, in fact, any literate persons who think that the world is out of joint and offer remedies that some editor will accept. For example, in the May issue of the SM appeared a review of a book by Roger J. Williams, a well-known chemist, on "humanics;" and in the same issue was reviewed a book by Lecomte du Noüy, an equally well-known biophysicist, on human destiny. No social scientist would write a book on a new system of organic chemistry; why, he must wonder fretfully, would a chemist write a book on social engineering? The answer, I think, is that everyone in a democracy is expected to have opinions on social questions. Lay opinions of prominent, intelligent citizens will continue to be published and read until social scientists prove by their works and the results of their works that they are as uniquely competent in their fields as organic chemists are in theirs.

I do not intend, as yet, to limit articles on social-scientific questions in the SM to those written by social scientists, but I do want to encourage them to write for the SM. I want to publish articles by them that will bear evidence of true research and that can thereby be distinguished from inspirational and philosophical writing backed by no conscious research. I want to help them both by getting better articles on social science and by excluding unworthy articles. To that end I have welcomed and accepted an offer of cooperation from the American Sociological Society. Dr. Louis Wirth, President of that society, has appointed the following special committee to advise me: Alfred McClung Lee, Wayne University, Chairman; Calvert L. Dedrick and Abraham Jaffe, Washington, D. C.

Finally, I am in favor of including a Division of Social Science in the proposed National Science Foundation. If social scientists can demonstrate that they are natural scientists, they can do so more effectively working with physical and biological scientists than apart from them.

F. L. Campbell

INDEX

CONTRIBUTORS

Principal Articles

ARMSTRONG, CHARLES: Memorial Laboratory, National Institute of Health, 365.
ASDELL, SYDNEY A.: The Future of the Science of Animal Breeding, 477.

BAILEY, HERBERT S., JR.: The University Presses—And the Popularization of Science, 416.
BENDER, JAMES F.: Aptitude Testing, 297.
BERLIN, LT. RICHARD B.: Impressions of Japanese Medicine, 41.
BOGGS, S. W.: Cartohypnosis, 469.
BRANDWEIN, P. F.: The Selection and Training of Future Scientists, 247.
BRIGGS, LYMAN J.: A 1947 Eclipse Expedition, 208.
BRODY, SAMUEL: The Nutritional Significance of Milk, 5.
BRUES, C. T.: Contributions of Entomology to Theoretical Biology, 123.
BUCKLEY, H.: The British National Physical Laboratory, 50.
BURCHAM, CAPT. L. T.: A "Grass-Counter" in the Pacific, 21.
BURR, H. S.: Field Theory in Biology, 217.
BUSWELL, G. T.: Perceptual Research and Methods of Learning, 521.

CAESAR, GEORGE V.: Private Flight to the Boston Meeting, 261.
CHAMBLISS, JOHN L.: A Layman Looks at Science, 464.
CHANDRASEKHAR, S.: The Story of Two Atoms, 313.
CHAPMAN, WILBERT MCLEOD: The Wealth of the Ocean, 192.
CHARDON, CARLOS E.: Naturalists in Tropical America, 198.
CHESNUTT, JAMES G.: *See* VALENTINE, WILLARD L.
COLE, IRWIN H.: *See* SHAPLEY, HARLOW.
COOKE, GILES B.: Progress in Cork Culture in the United States, 117.
CUMBERLEDGE, A. A.: Aerological Aspects of the Bikini Bomb Test, 135.

EMERSON, ALFRED E.: Why Termites? 337.

FAWCETT, C. B.: The Numbers and Distribution of Mankind, 389.
FLEMMING, ARTHUR S.: Scientists and the Civil Service, 515.
FOX, SIDNEY W.: Amino Acids, 103.

GARBER, CLARK M.: Eskimo Infanticide, 98.
GERARD, RALPH W.: The Scope of Science, 496.
GLICKSBERG, CHARLES I.: Henry Adams and the Repudiation of Science, 63.
GOSNELL, CHARLES F.: Obsolete Library Books, 421.
GUDGER, E. W.: The Only Known Fish-Catching Plant, 369.

HARROW, BENJAMIN: Enzymes, Vitamins, and Hormones, 242.
HEMENS, ROLLIN D.: The University Presses: Their Function, 412.
HERRICK, C. JUDSON: Seeing and Believing, 253.
HUMPHREYS, W. J.: All of a Kind, Caper to Canto, 211.

IVES, RONALD L.: The Rediscovery of Cup Butte, 33.

KELLER, W. D.: Acid Clay—A Weathering Agent, 513.
KETTERING, CHARLES F.: Thomas Alva Edison, 109.
KING, THOMSON: On Life as a Separate Entity, 161.
KIPP, LAURENCE J.: *See* SHAFFER, KENNETH R.
KROGH, AUGUST: The Production and Ripening of Red Blood Cells, 301.

LAWTON, GEORGE: Old Age: Minus and Plus, 53.
LUNDBERG, GEORGE A.: The Senate Ponders Social Science, 397.

MCCONNELL, ROBERT A.: A Physicist Looks at Morality, 61.
MARGENAU, HENRY: Particle and Field Concepts in Biology, 225.
MERRILL, PAUL W.: The Principles of Poor Writing, 72.

NEWELL, H. E., JR.: Exploration of the Upper Atmosphere by Means of Rockets, 453.

PALMER, KATHERINE VAN WINKLE: Fun with Fossils, 385.
PAYNE-GAPOSCHKIN, CECILIA: Harlow Shapley, 189.

SCATES, DOUGLAS E.: The Parallel Roles of Physical and Social Science, 14.
SHAFFER, KENNETH R., and KIPP, LAURENCE J.: Books—Agents of War and Peace, 428.

SHAPLEY, HARLOW: Liaison with Youth, 347.
SHAPPIRIO, DAVID G.: *See* SHAPLEY, HARLOW.
SHAW, HERBERT A., JR.: *See* VALENTINE, WILLARD L.
SIMPSON, GEORGE GAYLORD: The Problem of Plan and Purpose in Nature, 481.
STEINER, PAUL E.: Okinawa and Its People, I, 233; II, 306.
STIMSON, DOROTHY: A Case Report on a History of Scientific Ideas, 148.

THOMSON, J. W., JR.: How the Junior Academies of Science Operate, 327.
TUKEY, H. B.: 2,4-D, A Potent Growth Regulator for Plants, 93.

VALENTINE, WILLARD L.: Science Writing Awards for 1946, 75.
VAN DYKE, H. B.: The Weapons of Panacea, 322.

WAKSMAN, SELMAN A.: Microbiology in the U.S.S.R. in 1946, 289.
WESTGATE, LEWIS G.: Varied Approaches to Nature, 155.
WHITE, STEPHEN: *See* VALENTINE, WILLARD L.
WILLIAMS, ROGER J.: Humanics: A Crucial Need, 174.

ZAHL, PAUL A.: The Flamingos of Andros, 277.
ZIRKLE, R. E.: The Particle Physics Approach to Biology, 213.

Book Reviews

(Indexed by Reviewers)

ALLISON, IRA S.: *Dating the Past.* An *Introduction to Geochronology*, by Frederick E. Zeuner, 527.
ANDERSON, JOHN E.: *The Human Frontier*, by Roger J. Williams, 443.
AUTEN, JOHN T.: *Forest Soils and Forest Growth* by S. A. Wilde, 271.

BENJAMIN, A. CORNELIUS: *The Outlook of Science*, by R. L. Worrall, 86.
BERLIN, RICHARD: *Victory over Pain*, by Victor Robinson, 530.
BUCHANAN, J. W.: *The World Expands*, by George Howard Parker, 440.
BURCH, GUY IRVING: *India's Population*, by S. Chandrasekhar, 267.

CALLAHAN, HAROLD D.: *Radio's Conquest of Space*, by Donald McNicol, 531.
CARLSON, A. J.: *Science, Its Effect on Industry, Politics, War, Education, Religion, and Leadership*, by D. W. Hill, 441.

DUVALL, ADDISON M.: *The Biology of Schizophrenia*, by Roy G. Hoskins, 87.

EISLEY, LOREN C.: *The Lost Americans*, by Frank C. Hibben, 182.
ELDER, ALBERT L.: *Miracles from Microbes*, by Samuel Epstein and Beryl Williams, 181.

FENTON, WILLIAM N.: *The Last Trek of the Indians* by Grant Foreman, 529.
FRAZIER, CHESTER NORTH: *A History of the University founded by Johns Hopkins*, by John C. French, 435.

GRAUBARD, MARK: *New Worlds in Medicine.* Harold Ward, Ed., 360.

HEDGPETH, JOEL W.: *Natural History*, by Pliny, 269.
HENDRICKS, B. CLIFFORD: *The Best of Science Fiction.* Groff Conklin, Ed., 186.

JANIS, LEE: *Medical Education and the Changing Order*, by Raymond B. Allen, 183.

KAPPAUF, WILLIAM E.: *Dials and Flight*, by Assen Jordanoff, 434.
KITCHIN, PAUL C.: *Dentistry, An Agency of Health Service.* Malcolm Wallace Carr, Ed., 361.
KNOX, R. W.: *The Star Atlas and Navigation Encyclopedia*, by S. S. Rabl, 266.

LEWIS, OSCAR: *The Path of Science*, by C. E. Kenneth Mees, 437.
LUCKHARDT, ARNO B.: *Banting's Miracle*, by Seale Harris, 438.
LUNDBERG, GEORGE A.: *From Max Weber: Essays in Sociology*, translated, edited, and with an introduction by H. H. Gerth and C. Wright Mills, 270.

MCCRACKEN, EARL C: *Household Physics*, by Madalyn Avery, 534.
MANN, WILLIAM M.: *Driftwood Valley*, by Theodora C. Stanwell-Fletcher, 268; *The Natural History of Nonsense*, by Bergen Evans, 358; *The Gardener's Bug Book: 1,000 Insect Pests and their Control*, by Cynthia Westcott, 439; *Audubon Bird Guide: Eastern Land Birds*, by Richard H. Pough, 533.
MATHER, KIRTLEY F.: *Human Destiny*, by Lecomte du Noüy, 447.
MILLS, JOHN: *Charles Darwin and the Voyage of the Beagle.* Lady Nora Barlow, Ed., 356.

O'BRIEN, H. R.: *Health Instruction Yearbook, 1946*, by Oliver E. Byrd, 362.
OEHSER, PAUL H.: *The Symbolic Goldfinch: Its History and Significance in European Devotional Art*, by Herbert Friedmann, 356; *The Colorado*, by Frank Waters, 532; *Listen, Bright Angel*, by Edwin Corle, 532.

POWERS, PHILIP N.: *Science and Freedom*, by Lyman Bryson, 446.

RULE, GLEN K.: *Drought, Its Causes and Effects*, by Ivan Ray Tannehill, 528.

SANDERS, PAUL D.: *Research and Regional Welfare*, 265.

SCHMIDT, KARL P.: *The Wolf in North American History*, by Stanley Paul Young, 445.

SHIMKIN, MICHAEL B.: *New Aspects of John and William Hunter*, by Jane M. Oppenheimer, 359; *The Endeavour of Jean Fernel*, by Sir Charles Sherrington, 359.

SMITH, C. M. *Scientific Instruments*, by Herbert J. Cooper, 88.

STEBBINS, JOEL: *The Earth and the Stars*, by C. G. Abbot, 353.

STIRLING, M. W.: *The Ancient Maya*, by Sylvanus G. Morley, 433.

TUKEY, H. B.: *General Horticulture*, by Thomas J. Talbert, 269.

WESTHEIMER, FRANK H.: *What Industry Owes to Chemical Science*, A Symposium, 439.

WOLFLE, DAEL: *Executive Ability: Its Discovery and Development*, by Glen U. Cleeton and Charles W. Mason, 354.

WYMAN, LELAND C.: *The Navaho*, by Clyde Kluckhohn and Dorothea Leighton, 442.

Verse

ALMQUIST, ARDEN: To a Cadaver, 346.

BARBOUR, ALMON: Prospect and Retrospect: A Dream of Man, 160.

GOODMAN, MAY WINKLER: Eclipse, 246; Butterflies, 520.

LAWSON, DOUGLAS E.: Genesis of Words, 476.

LOUGEE, F. MARION: Absorption Spectra, 305.

MUNZER, MARTHA E.: Plutonium—Named for the God, 326.

NEWBERRY, WILLIAM: Beyond Time and Space, 108.

SINCLAIR, JOHN G.: Heart or Head, 74.

SMITH, T. V.: War Echoes of Lucretius, 71.

SMYTHE, DANIEL: At the Airport, 526.

WHITNEY, BARBARA: Unnumbered Springs, 396.

Comments and Criticisms

ANDERSON, RICHARD: Toward a World State, 187.

BLAKE, L. V.: In the Negative, 449.

CAMPBELL, F. L.: The Brownstone Tower: 92, 188, 276, 364, 452, 536.

CLEMENT, A. W.: Undismayed, 451.

CORLETT, EDWIN HOWARD: The Carolina Bays, 187; On the Mathematics of Committees, Boards, and Panels, 187.

DEWAR, ROBERT KERR: The Big Ten, 449.

DURHAM, O. C.: Pollen Counts, 535.

DURYEA, CHESTER B.: Justly Cynical, 275.

GIVLER, ROBERT C.: Extremes, 363.

HOEBEL, E. ADAMSON: Eskimo Infanticide and Polyandry, 535.

HOLROYD, HOWARD B.: To Write or Not to Write 451.

HOLTAN, VICTOR: The Golden Rule, 363.

JOHNSON, CHARLES F., JR.: Appreciation, 535.

KEPNER, WILLIAM ALLISON: Imponderables, 450.

KLAUDER, RUDOLPH H.: Parapsychology, 274.

LAMBERT, JOSEPH M.: Information, Please, 363.

MOLINA, EDWARD C.: Mathematical Allusions in Poe, 89.

MORRIS, DANIEL LUZON: Extremes, 363.

MUELLER, GUSTAV E.: Zeno and the Mathematician, 273.

RAFTON, HAROLD R.: Humanism, 89.

SAMACHSON, JOSEPH: Science Books for the Non-scientist, 91.

SOLEY, W. G.: Carborundum, 451.

SUTER, RUFUS: Important Books, 275.

TWINING, HARRY LA VERNE: How About Mind? 450.

WATSON, J. M.: Disappointment, 275.

WOODCOCK, H. E.: Atomic War, 274.

SUBJECT INDEX

Adams, Henry (CHARLES I. GLICKSBERG) 63.

Aerology (A. A. CUMBERLEDGE) 135.

American Book Center (KENNETH R. SHAFFER and LAURENCE J. KIPP) 428.

Animal breeding (SYDNEY A. ASDELL) 477.
Aptitude testing (JAMES F. BENDER) 297.
Astronomical physics (S. CHANDRASEKHAR) 313.
Astronomy (LYMAN J. BRIGGS) 208; (S. CHANDRASEKHAR) 313.

Bikini (A. A. CUMBERLEDGE) 135.
Biography (CECILIA PAYNE-GAPOSCHKIN) 189; (CARLOS E. CHARDON) 198.
Biophysics (RAYMOND E. ZIRKLE) 213; (H. S. BURR) 217; (HENRY MARGENAU) 225.
Bladderwort (E. W. GUDGER) 369.
Books, destruction of (KENNETH R. SHAFFER and LAURENCE J. KIPP) 428.
Books, obsolescence of (CHARLES F. GOSNELL) 421.
Boston meeting, A.A.A.S. (GEORGE V. CAESAR) 261.
Botany (L. T. BURCHAM) 21; (H. B. TUKEY) 93; (GILES B. COOKE) 117.

Chemistry (SIDNEY W. FOX) 103; (BENJAMIN HARROW) 242.
Civil service (ARTHUR S. FLEMMING) 515.
Cork culture (GILES B. COOKE) 117.
Cup Butte (RONALD L. IVES) 33.

Eclipse of the sun (LYMAN J. BRIGGS) 208.
Edison, Thomas Alva (CHARLES F. KETTERING) 109.
Education (DOROTHY STIMSON) 148; (PAUL F. BRANDWEIN) 247; (JOHN W. THOMSON, JR.) 327; (G. T. BUSWELL) 521.
Electronics (IRWIN H. COLE) 351.
Entomology (CHARLES T. BRUES) 123; (ALFRED E. EMERSON) 337; (IRWIN H. COLE) 351.
Erythrocytes (AUGUST KROGH) 301.
Eskimos (CLARK M. GARBER) 98.
Ethnology (CLARK M. GARBER) 98; (PAUL E. STEINER) 233, 306.
Evolution (GEORGE GAYLORD SIMPSON) 481.

Fish-catching plants (E. W. GUDGER) 369.
Flamingos (PAUL A. ZAHL) 277.
Fossil collecting (KATHERINE VAN WINKLE PALMER), 385.

Hay fever (HERBERT A. SHAW, JR) 79.
Herbicides (H. B. TUKEY) 93.
History of science (DOROTHY STIMSON) 148; CARLOS E. CHARDON) 198; (H. B. VAN DYKE) 322.

Infanticide, Eskimo (CLARK M. GARBER) 98.
Isotopic tracers (RAYMOND E. ZIRKLE) 213.

Maps (S. W. BOGGS) 469.
Mathematics (IRWIN H. COLE) 351.
Medicine (CHARLES ARMSTRONG) 365.
Medicine, Japanese (RICHARD B. BERLIN) 41.
Meteorology (A. A. CUMBERLEDGE) 135.
Microbiology (SELMAN A. WAKSMAN) 289.
Milk (SAMUEL BRODY and DULAL PADA SADHU) 5.

Natural history (CARLOS E. CHARDON) 198; (E. W. GUDGER) 369; (KATHERINE VAN WINKLE PALMER) 385.
Nature (LEWIS G. WESTGATE) 155.
Nutrition (SAMUEL BRODY and DULAL PADA SADHU) 5.

Ocean resources (WILBERT MCLEOD CHAPMAN) 192.
Okinawa (PAUL E. STEINER) 233, 306.
Old age (GEORGE LAWTON) 53.
Operation Crossroads (A. A. CUMBERLEDGE) 135.
Ornithology (PAUL A. ZAHL) 277.

Particle physics (RAYMOND E. ZIRKLE) 213; (H. S. BURR) 217; (HENRY MARGENAU) 225.
Pharmacology (H. B. VAN DYKE) 322.
Philosophy (GEORGE LAWTON) 53; (ROBERT A. MCCONNELL) 61; (CHARLES I. GLICKSBERG) 63; (LEWIS G. WESTGATE) 155; (THOMSON KING) 161; (ROGER J. WILLIAMS) 174; (W. J. HUMPHREYS) 211; (C. JUDSON HERRICK) 253; (RALPH W. GERARD) 496; (JOHN L. CHAMBLISS) 464.
Plague (JAMES G. CHESNUTT) 76.
Plant physiology (H. B. TUKEY) 93.
Population (C. B. FAWCETT) 389.
Psychology (GEORGE LAWTON) 53 ; (ROGER J. WILLIAMS) 174; (JAMES F. BENDER) 297; (G. T. BUSWELL) 521.

Radar (STEPHEN WHITE) 83.
Reading methods (G. T. BUSWELL) 521.
Research, British (H. BUCKLEY) 50.
Rockets (H. E. NEWELL), JR.) 453.

Satire (PAUL W. MERRILL) 72.
Science legislation (GEORGE A. LUNDBERG) 397.
Science Talent Search (HARLOW SHAPLEY) 347.
Science Writing Awards (WILLARD L. VALENTINE) 75.
Shapley, Harlow (CECILIA PAYNE-GAPOSCHKIN) 189.
Social science (DOUGLAS E. SCATES), 14; (GEORGE A. LUNDBERG) 397.
Soils (W. D. KELLER) 513.
Southwest Pacific (L. T. BURCHAM) 21; (PAUL E. STEINER) 233, 306.

University presses (ROLLIN D. HEMENS) 412; (HERBERT S. BAILEY, JR.) 416.
U.S.S.R. (SELMAN A. WAKSMAN) 289.

...a Kodak color film you can process yourself

• Ektachrome Film meets the need of the worker who wants to compare the finished photographic result in full color with his specimen while it is still set up.

The user of Ektachrome processes it in his own darkroom in ninety minutes, without special equipment and with materials just as safe as ordinary developers. He obtains a positive transparency from which color prints can be made if wanted.

Ektachrome comes in the same sheet sizes as Kodachrome Professional Film, in both daylight and Type B (3200°K. source) color balance with approximately the same emulsion speeds.

Ektachrome Film and Ektachrome Processing Kits with instructions are now available from your Kodak dealer.

EASTMAN KODAK COMPANY
ROCHESTER 4, N. Y.

Kodak

General Library,
University of Michigan,
Ann Arbor, Mich.

RCA Communications' new world-wide automatic tape relay radio system speeds Radiograms.

New wings for words around the world!

Radiograms "Via RCA" to and from overseas points now are processed by automatic machines which speed your messages through such gateway cities as New York, London, San Francisco and Manila, without delay.

This advanced technique in international radiotelegraphy is the result of wartime research and development. It gives to private messages the same speed, accuracy and dependability which were attained through its use by the U. S. Army Communications Service during the war.

RCA Laboratories—one of the world's foremost centers of radio and electronic research—is continually pioneering and advancing radio communications in service to the Nation and the public.

When you buy an RCA Victor radio or television receiver, Victrola radio-phonograph, or phonograph record, you are getting, thanks to RCA research and engineering, one of the finest products of its kind science has achieved.

"Victrola" T. M. Reg. U. S. Pat. Off.

Radio Corporation of America, RCA Building, Radio City, New York 20. Listen to the RCA Victor Show, Sundays, 2:00 P. M., Eastern Daylight Saving Time, over NBC.

At RCA Communications, "Package Sets" contain an automatic sending and receiving unit for a foreign gateway city. Messages in tape form and received through these machines, are ready for delivery or immediate transmission to any part of the world.

RADIO CORPORATION of AMERICA